A CRITICAL EVALUATION OF RECENT RESEARCH INTO SEMANTIC DEVELOPMENT IN CHILD LANGUAGE

ROY THOMAS ALAN PICKERILL

Thesis presented for the Degree
of
Master of Arts

University of Cape Town.
August 1988.

ABSTRACT

This study examines critically recent research in the area of child language development, with an emphasis on research into semantic development. Various research articles, in particular, are analysed, with particular attention being given to experimental studies. In addition, research into language development in the naturalistic mode is discussed.

The validity of research into language development in experimental contexts is questioned. Specific methods employed in experimental studies of language development are discussed critically. These methods are contrasted with methods used in a number of naturalistic studies. Recent research into semantic development is placed in the perspective of the study of semantics as a whole.

The principal finding of this study is that research into language development in artificial experimental settings does not allow for valid conclusions to be drawn. Naturalistic studies are preferred in that they allow for language development, and semantic development in particular, to be placed in the context of overall child development.

Language development is not able to be abstracted, for research purposes, from the totality of human development. Semantic development is viewed as a continuous process, lasting well into adulthood. The importance of the study of semantic development as part of semantics is emphasised.

ACKNOWLEDGEMENTS

I should like to thank the following:

(i) Professor Roger Lass, for his encouragement which helped me to get started;

(ii) Dr Nigel Love, for his guidance, constructive criticism and the prompt way in which he attended to the task of supervision of this thesis;

(iii) Brenda Beneke for typing this thesis with speed, patience and cheerfulness;

(iv) my parents, family and friends for their help and sustained interest in my research.

Roy Pickerill
1988

CONTENTS

Chapter 1

INTRODUCTION

1. The influence of Noam Chomsky on research into child language

There has been rapid growth in the study of child language over the last three decades. Impetus for research has come from a number of sources, but one of the main influences appears to have been the writings of Noam Chomsky in linguistics.

The publication of Chomsky's Syntactic Structures in 1957 introduced a rigorous mathematical formalism into linguistics. Chomsky laid down principles which he thought should govern the study of language, and grammar in particular. His Aspects of the Theory of Syntax (1965) developed the themes of his earlier book. Chomsky set out a theory of phonology in his joint work with Morris Halle, The Sound Pattern of English (1968). Chomsky has, however, not developed a theory of semantics in as much detail as his theories of syntax and phonology.

Psychologists were already studying child language as a component of human behaviour when Chomsky's (1959) critique of B.F. Skinner's (1957) Verbal Behavior was published. Chomsky's debate with Skinner, a psychologist and proponent of behaviourism, enhanced the interest of researchers in child language. Psychologists such as Roger Brown and Dan Slobin became

involved more deeply in the study of child language and applied the existing tools of research psychology in their work. Thus children were tested in experimental situations and their linguistic behaviour was analysed, usually with the aid of statistics.

Chomsky suggested the topic of his wife's experimental study on child language, The Acquisition of Syntax in Children from 5 to 10 (1969), the contents of which has proved a fruitful source for subsequent researchers. In his enlarged edition (1972) of Language and Mind, Chomsky makes an important statement on the relationship between linguistics and psychology:

"I think there is more of a healthy ferment in cognitive psychology - and in the particular branch of cognitive psychology known as linguistics - than there has been for many years" (1972:1).

This statement would appear to have channelled the interest of researchers in psychology into the study of linguistics, and of child language in particular.

Two years after Chomsky's 1972 version of Language and Mind was published, the Journal of Child Language came into being. It provided a forum for linguists and for psychologists interested in linguistics to publish their research into child language. Much of the important research work on child language since 1974 is contained in this journal. Its articles on semantic development, for instance, cover most of the research

and bibliographies of research into the area of semantic development. Accordingly these articles form the bulk of the research material for this thesis. Consideration will also be given to research on semantic development in children which has been published elsewhere. The emphasis will be on a critical evaluation of the research methods that make use of structured tests.

The scope of this thesis, then, is the study of published recent research into the development of semantics in child language, and the study of relevant articles in the Journal of Child Language in particular. The thesis will attempt to provide guidelines for further research into semantic development, but will not concentrate on an evaluation of different theories of semantic development, as much evaluation of these already exists, particularly in the work of Atkinson (1982) and of Elliot (1981).

In brief, the chief influences on recent research into child language, and hence semantic development as part of this field appear to have been :

(a) Noam Chomsky's theories, more particularly his views on first-language acquisition;

(b) the aims of psychologists studying child language as part of human behaviour;

(c) the work of linguists (often in conjunction with psychologists) with a special interest in child language.

The view that linguistics is a branch of cognitive

psychology, as expressed by N. Chomsky (op. cit.), has exerted an influence on research methods employed in the study of child language.

2. Recent research on child language

Although mention will be made of research on child language prior to 1970, this thesis will concentrate on work published after that date, and on published experimental work in particular. The main sources that have been used to narrow down the field of study are :

(a) most of the noteworthy research on child language listed in the bibliography in Elliot's book, Child Language (1981);

(b) articles in the Journal of Child Language (JCL) particularly those relevant to semantics;

(c) articles in journals other than the JCL, such journals having gained mention either in Elliot (1981) or in the various references contained in the JCL;

(d) chapters in books published since 1970 in which there is a critical discussion of research into semantic development in child language.

Apart from the JCL, the periodicals that contain most of the relevant research for the purposes of this thesis are : the Journal of Verbal Learning and Verbal Behavior (JVLVB), Word, Child Development, the Journal of Psycholinguistic Research (JPR), Cognition, the British Journal of Psychology, the Journal of Experimental Child Psychology and the International Journal of Psycholinguistics. By far the most

relevant, however, are the JCL, the JVLVB and the JPR.

Prior to the founding of the JCL (in 1974) most of the published research on child language was to be found in the JVLVB. Articles on child language had also appeared in various education, psychology and medical journals. It appears that the research methods reported in psychology journals, in particular, had a powerful influence on researchers into child language. The Journal of Psycholinguistic Research was first published in 1972 and contains a number of articles on child language. The volume of published research on child language showed rapid growth after 1970. It would appear that the JCL came into existence as a result of this growth. It is appropriate to take a close look at the founding aims of this periodical.

The Journal of Child Language was first published in 1974, under the editorship of David Crystal. In his opening editorial, Crystal mentioned "three emphases in particular which we would like to see develop" (1974:(ii)). The first of these was the need for the journal "to contain a regular proportion of papers on languages other than English" which would provide "testable or tested hypotheses" in respect of "linguistic universals of development". The second emphasis would be on "a diversity of theoretical approaches to child language" since there appeared to be a lack of freshness in the theoretical debate, with

researchers taking up polar stances on the theoretical models (particularly generative theory). The third emphasis would be on "methodological issues, which have tended to be neglected in recent work in linguistics". Such issues deserved attention in the study of child language "where the role of adult intuition is so debatable". Crystal also stressed the following:

"Analytic criteria need to be fully explicit, if one is to evaluate claims about when a rule can be said to be 'acquired', for example, or to ensure that comparative and typological work is carried out consistently. We feel that questions of experimental design, evaluation, terminology and notation have been insufficiently aired in relation to the idiosyncrasies of child language data, and we look forward to contributions concerning these matters" (1974:(ii)).

It has already been stated, in the first two sections of this chapter, that the research methods of psychology have influenced the methods of psycholinguists. The basic method of psychological research is the structured test (experiment or interview), often carried out on a group or sample of subjects, as in Chomsky (1969), and usually accompanied by statistical analysis. While this thesis will not concern itself directly with a critique of the statistical aspects of tests of language development, it is appropriate to comment briefly on these, since

they often form part of tests discussed both in the JCL and elsewhere. After the discussion of the merits of statistics in tests of language development, the main concerns of the thesis proper will be introduced.

3. The use of statistical methods in tests of language development

The growth of mathematical statistics as a science since 1950 also exerted a strong influence on all research involving measurement. Hypothesis-testing using statistical methods became very commonplace. Tests involving statistics enjoyed a wide use and acceptance in psychology. It is natural that child language researchers, many of whom are psychologists, should have adopted these tests.

Statistics has provided useful decision-making tools in certain sciences, especially those in which there are standard units of measurement. In certain areas of research into language, statistical methods have proved valuable: for example, in research into sound frequencies in phonetics. Human speech falls into a certain segment of a frequency spectrum so that, in this field, the use of units of measurement (e.g. hertz) and techniques of sampling and testing do appear to have theoretical validity. In syntactic studies there are elements of predictability as regards word order and frequency of occurrence. Here, too, statistical methods may prove useful both descriptively

and inferentially. But it is in the area of semantics that the use of statistics appears to be the most questionable, for here the 'fuzziness' of the subject makes for difficult analysis.

The use of statistical methods to make inferences about populations on the basis of samples is widespread. However, there is an element of uncertainty involved in such procedures, as Mood and Graybill emphasise :

"There are two types of uncertainty with which we must contend : (1) uncertainty due to randomness and (2) uncertainty due to our ignorance of the true state of the system" (1950:1).

This uncertainty increases if the researchers who are using statistical methods do so without the assistance of a statistician. Very few research articles containing statistical tests in the JCL, for instance, mention that any such assistance has been given. This practice is unfortunate for (at least) two reasons : first, the reader is left not knowing the extent to which the writer(s) of these articles satisfied themselves of the appropriateness of the tests which were used. The second reason is that writers of subsequent articles tend to be influenced by the degree to which reference to statistical advice is made by their predecessors. A lack of reference may tend to become accepted as the *norm*, to the detriment

of the overall research method.

Many articles in the JCL include statistical tests on small numbers of children. This has the effect of weakening the evidence which the tests are supposed to provide. Large samples are in themselves no guarantee of achieving satisfactory results. They do, however, tend to swing the argument in favour of the researcher, since they are an indicator of his overall thoroughness. The size of the sample varies with the type of statistic used in the test, but in most cases tests carried out with small samples (usually lower than 30) should be viewed very cautiously.

One of the factors that reduces sample size is the difficulty of obtaining data from the sample of children originally selected. Children may be ill on the day of the test, or unavailable for a number of other reasons. In some cases, the child may be uncooperative or unresponsive. Children selected for testing longitudinally, i.e. over a specified time period, may move with their parents to a distant suburb or a different town. Parents may decide that they no longer wish their children to be tested. Thus, the sample size may reduce, even drastically, from the original. Researchers who have invested much time and effort may press on with the smaller sample, thus weakening their results. It would appear that the stated aim of the JCL of strengthening research

methodology has not necessarily been achieved in respect of the use of statistics.

Statistical analysis has nevertheless become established as part of structured tests of language development (LD). What is of more concern, in this thesis, however, is the *structured* nature of the tests themselves. This aspect will now be discussed, as it is crucial in any analysis of research into language development.

4. The main concerns of this thesis

Against the background of the main influences on research into LD, and on its subsequent growth, it is the central concern of this thesis to analyse critically the methods used in recent research in the field, and in semantic development in particular.

The main purpose of this thesis is to show that structured tests of LD are essentially artificial in their design and execution and that, as such, are inappropriate and lacking in validity as indicators of LD, since LD is viewed essentially as a *natural* process.

The question of the use of statistics in tests of LD has already been discussed. It is claimed that this aspect of research into LD is over-emphasised and can potentially do more to weaken than to strengthen the researcher's case. In the chapters which follow, various aspects of the artificiality of LD tests will

receive attention. It is claimed that the following aspects of LD tests are artificial and tend to be invalidating :

(a) the environment and overall test design, including the duration of the test and the methods of recording test data;

(b) the props and materials used in tests;

(c) the language used in the design of test items.

An intensive study will also be made of tests of relative clause acquisition, in order to exemplify how the artificiality of test situations tends to invalidate the results of these tests.

As a foil to the methods used in structured (experimental) tests of LD, a number of naturalistic studies in semantic development will be analysed. A further hypothesis is that these methods, being naturalistic, are concordant with the aim of obtaining knowledge about the way in which language *develops* in children.

Finally, space will be devoted to a concluding comparison of the methods of the experimental and the naturalistic approaches, as well as to a consideration of the importance of the study of semantic development to the study of semantics as a whole.

For ease of reference, the gender of the *child*, unless disambiguated by the context, will be taken as male. In much of the literature, children are studied

with reference to their mothers, so that the latter convention allows for variety in the use of pronouns. The gender of the researcher will usually be clear from the context of the research under discussion. The gender of the writer (of an experimental research article) will usually also be assumed to be the gender of the "experimenter".

A comment is in order concerning the notation followed by child language researchers in respect of the age of child subjects. Age is generally stated as: years; months. For example, an age of two years and seven months is written as 2;7.

Chapter 2

A DISCUSSION OF THE FACTORS IN TESTS OF LANGUAGE DEVELOPMENT WHICH TEND TO DISTORT AND INVALIDATE THE RESULTS OF THESE TESTS

In this chapter the question of the testing of children's language development in an artificial situation is discussed. It will be claimed that a number of factors are present in such situations which tend both to distort the results and to prevent a generalisation of the results to other situations, in which the children's daily routine can be contrasted to that of the test. Several of the tests under discussion involve semantic development.

The situation of a test of language development is artificial in that it brings with it a number of non-routine features. In the first instance, it appears likely that the children being tested will feel a certain amount of anxiety: to a greater or lesser extent they will realise that they are being expected to render a performance for the benefit of the tester, who is usually a strange adult. Since the behaviour of children (like all humans) is context-dependent (Romaine, 1984:14), it follows that experimenters need to be aware of the effects of the context of the test on the behaviour (linguistic or otherwise) of the children being tested.

The presence of a strange adult may influence a child to be less responsive than usual, or it may have the effect of making the child "show off" his knowledge. Not only does

an adult tester exercise influence as an observer, but as an adult he (or she) is likely to be perceived as an authority-figure by the child. The child's verbal responses are likely to be modified *because* there is an adult observer present. Thus the presence of an adult observer in itself acts to distort the observations which are being sought – the Observer's Paradox referred to by Romaine (1984:16) in her discussion of methods of testing.

To circumvent the problem of his or her strangeness to the children being tested, it is a moderately common practice for an experimenter to spend a period of time familiarising himself with the children. This is often done in the child's home, with the mother (and sometimes the father) present. There is a lack of consistency among researchers in respect of this practice: where it fails to gain mention in the literature, it is likely to have been omitted. It is unfortunate, in attempting to compare the findings of different researchers, not to know whether they employed a period of familiarisation with their subjects.

That a formal testing situation places a certain amount of strain on child subjects is evident from an analysis of a number of published research findings. Chambers and Tavuchis (1976:66) refer to the strategy of incorporating questions of children's understanding of kinship in a *game* "in order to avoid putting the child into a traditional testing situation and to help maintain interest and attention". Haviland and Clark (1974) emphasise the relaxed

and informal nature of their interviews with children. Thomson and Chapman (1977) report cases of children being dropped from a study because of their failure to participate consistently in a picture-labelling test, and even failing to respond at all. Tyler and Marslen-Wilson (1978) tested children using tape-recordings made by a female reader who read with a natural intonation pattern. This strategy was apparently used to approximate to the child's hearing of his mother's voice. Once again, the extent to which different children show signs of anxiety is seldom emphasised in the literature, making inter-study comparisons difficult.

A number of researchers report that their child subjects became bored during tests. Kuczaj and Lederberg (1977:407) mention the case of one child (in a group of 45 being tested for their comprehension of the words <u>younger</u> and <u>older</u>) who "became so bored with the situation that two sessions on different days were required". The particular test consisted of first showing the child 16 pairs of objects and asking him three questions about each pair, then following this with more questions about another four pairs of objects. Since some of the children being tested were only four years old, a series of well over 50 questions would be likely to lead to difficulties in obtaining responses. Chambers and Tavuchis report in their research on the understanding of kinship that children aged about 6;0 were

> "reasonably attentive and co-operative, although still

prone to making errors or becoming distracted" (1976:65).

It does not appear that the effects of test-induced boredom on the results obtained in studies of language development have been adequately researched. Certainly, there is wide variation in the duration of tests administered to children of a given age. For a group of two-year olds, the work of Thomson and Chapman (1977) reveals that testing sessions had to be terminated after half an hour to an hour, owing to the child's becoming tired. Fremgen and Fay (1980) tested 16 children in the children's own homes, in a single session lasting 45 minutes. These children were aged between 1;2 and 2;2. McDonald and Pien (1982) report on a 30-minute session with mother-child pairs where the children were aged 2;5 to 3;0. One of the subjects became restless after 25 minutes. Angiolillo and Goldin-Meadow (1982) administered a test lasting 1 1/2 - 2 hours to children aged 2;4 - 2;11 1/2.

Apart from these studies, there have been cases where children have been dropped from a research group, often with no reason given. It seems likely that many children at the younger ages are not able to concentrate for more than 30 minutes in this type of test situation. A glaring omission from a large number of tests of language development is any statement of the average duration of the test. If researchers are finding that certain types of test become problematic because of the children's short attention span, then it would seem that this difficulty must surely be

widespread. An attempt needs to be made by researchers to set age-related time limits for structured tests, in the light of what current experience has revealed about the attention-span of children.

In studies involving the naturalistic observation of children, the time factor does not appear to be as important, presumably because the children can transfer from one type of activity to another when it suits them. Also, in naturalistic studies mothers are normally present, so the child tends to follow his daily routine, which acts as a reassuring factor.

In general, most of the tests of language development cited in the literature do not last longer than two hours. Whether or not a time limit is an accurate reflection of the actual time spent in each case, is not really clear. There is a large element of vagueness in time-reporting as well, with the phrase "one to two hours" being used frequently. This is indicative of a weak method: greater accuracy is needed in recording the actual duration of tests.

A further question which needs to be addressed in research methodology is that of the physical environment of the test. This can vary considerably: in some cases the child's home is used, but usually it is more convenient to test children in a school (or nursery school) environment. By working in the latter environment, a tester is able to test a large number of children in a comparatively short time. The children are familiar with the school environment

and, to this extent, it is a natural one for them. Nevertheless, children realise that the test is in some way "special", and it would seem that this consciousness on their part may have a bearing on the outcome. It is instructive to examine a number of different testing environments and to compare them critically.

Coker (1978) refers to individual testing of children as part of a study of syntactic and semantic factors in the acquisition of before and after. Children aged between 5;3 to 7;7 were tested in "a mobile laboratory for one or two sessions of about twenty minutes each. After every six to eight trials the child was given the choice to continue or not." The strangeness of the mobile laboratory could have had an inhibiting effect on the responses of children who were tested in this way. The novelty of the environment would capture the child's interest and would hold potential for distraction from the subject-matter of the test itself.

In another series of tests, Tyler and Marslen- Wilson (op. cit.) carried out experimental sessions lasting twenty minutes, with the children wearing headphones and being tested individually in a quiet room. These children were in three groups of ten aged, on average, five, seven and eleven respectively. The older children would probably not view this situation as being as strange as the youngest group would. The tests in question dealt with sentence processing and memory. Apart from differences arising on account of older children having more experience of sentence

processing, it is arguable that the greater maturity of the older group would improve their chances of concentration on the task at hand, under these conditions. The age factor alone should not be seen by the researcher as the main difference among the three groups : the fact that the younger groups may find the physical conditions of the test more distracting than the older group could have a bearing on the outcome. If this were so, then the test would not, in effect, be the *same* for the three groups.

Another investigation in which a quiet room was used was carried out by Emerson (1980). In this case, children were tested individually in the spare room of a school. These children, being five years and older, would be likely to have at least seen this room or to have known of its existence. But it would nevertheless be somewhat strange and its use could influence the children's responses. Feagans (1980) used a quiet room of a school and Norlin (1981) used quiet and well-lighted rooms of schools. Johnston (1984) used small rooms in a nursery school. It appears that many interviews and tests are carried out in quiet rooms of nursery and other schools, whereas, for most children, the nursery school is a place where activity takes place in noisy and often crowded rooms. By taking children out of their normal playrooms into a quiet room, often containing strange equipment, the interviewer is introducing factors into his test which could distort the data which is obtained.

Taking a different approach, Golinkoff and Markessini (1980) tested five groups of six children aged 1;7 to 5;5. The children's mothers were used to help with the test, which was performed in a small room with toys and child-size furniture (table and chairs). The presence of their mothers, as well as toys and more familiar furniture could lead to more natural responses from the children being tested. McDonald and Pien (1982) also used mother-child pairs - in this case the children were aged 2;5 to 3;0 - and carried out their observations in a carpeted playroom with one-way mirrors. Also in this vein, Corrigan and Odya-Weiss (1985) recorded 48 two-year olds on videotape in a playroom with a small table and chair, with one or both of the child's parents present. The use of parents during a test makes for a more natural environment, besides which the parents have extensive experience in communicating with their child and can therefore assist the tester to put ideas to the children and interpret their response. The presence of one or more parents during research into language development has become more commonplace in recent times.

It is instructive to examine the influences of other aspects of the test environment, viz. recording devices of the audio or video variety. In the earlier work on language development, during the period between 1960 and 1980, extensive use was made of tape-recorders. Insofar as it is not part of a child's everyday routine to have his speech recorded - in fact many children would regard such an event

as unusual - it could have the effect of making children "show-off" in their speech. The excitement caused by the novelty of the recorder could also distract the children from the questions being put to them by the interviewer. The novelty of a tape-recorder would be diminished if the children being tested were allowed to familiarise themselves in advance with the experience of having their speech recorded. This does not appear to be done very often, however.

In one case cited in the literature (Johnston, 1984), children of mean age 5;2 undergo tests lasting an hour, on a once-off basis, with a table microphone being placed one foot to one and a half feet from the child. In a long test of this nature, in a strange situation, it would seem that the children being tested would be likely to become distracted by the presence of the microphone.

Although many researchers, like Coker (1978), Emslie and Stevenson (1981) and Howe (1981), use tape-recorders, it is not clear in a large number of other studies whether a recorder has been used. It seems desirable that a researcher should always report on this aspect of his work, so that more comparative data could be assembled. The comments made on tape-recorders would also be applicable to video-recorders, possibly even more so. Because of the expense involved in the use of the latter, their occurrence in the literature is infrequent, however. Rodgon et al (1977) report on children being *unobtrusively* videotaped (my

emphasis).

Gordon Wells, by way of contrast, decided to incorporate a tape-recorder into a child's daily routine to allow for naturalistic observations of speech. A radio microphone was strapped inside the child's clothes and it was light enough for the child to carry about. Wells describes his method as follows:

"The recordings were made in the homes of the children by means of a radio-microphone linked to a battery-operated tape-recorder, controlled by a pre-set programme to switch on for short periods at intervals over a whole day. This technique makes it possible to collect samples of spontaneous speech which reflect the child's natural interaction with the environment, uncontaminated by the presence of an observer. Contextual information was obtained by replaying the tape to the mother in the evening of the same day and asking questions about the locale, participants and activity in each recorded period" (1974:256).

This method could not be used equally easily by all researchers; however, it does highlight the importance of obtaining natural speech from the child. The implications for structured tests would seem to be that the observer should be as unobtrusive as possible, and that all recording devices should be kept out of the way. There are now minature tape-recorders which can easily be hidden from the child's view, and these should be used when recording is

necessary. Large video cameras should be concealed, either behind a partition or in an adjoining room, if a laboratory is available.

In the more recent research, a number of methods of screening children prior to tests of language development are reported. Johnston (1984) screened a group of children with respect to their hearing and speech intelligibility. Conner and Chapman (1985) mention that the children were screened for sensory and cognitive functioning and that only children who were within normal limits were tested. These researchers also report (1985:116) on the children's "growing trust" in the instructions of the investigator. Norlin (1981) employed screening techniques, among which was a hearing test. A number of other researchers mention that children failed the initial stages and were then dropped from the test. The extent to which children perform poorly on tests of this nature because of hearing defects or cognitive problems is still unknown, because of the lack of reported research in this area.

So far, a number of factors relating to method have been cited as being problematical in research into language development. It is instructive to examine critically the methods used in a particular research study, bearing these previously mentioned factors in mind. The research to be discussed was carried out by Haviland and Clark. The authors studied the acquisition of English kin terms and sought to elicit definitions for 15 of these, using an

interview procedure "modelled on that of Piaget" (1974:36). Their aim was to interview 50 children between the ages of 3;0 and 8;10. However, only 30 complete protocols were obtained, from children between the ages of 3;5 and 8;10. The authors do not shed much light on the reasons for the other 20 protocols not being complete, and one is left to speculate. The interview method is described as follows:

" .... the first author interviewed 50 children ranging in age from 3;0 to 8;10, from the Bing Nursery School, Stanford. The interview procedure was modelled on that of Piaget (1928). The children were asked 'What is a mother?', 'What is an uncle?' etc., until definitions had been elicited for all 15 kin terms covered by this study (mother, father, grandmother, grandfather, son, daughter, grandson, granddaughter, brother, sister, aunt, uncle, niece, nephew and cousin). To control for any possible sequential effects, the order in which kin terms were asked for by the interviewer was varied by using 12 different permutations of the 15 terms" (1974:36-7).

The interviews were carried out in a quiet corner of the classroom and were relaxed and informal. Whenever possible, the interviewer tried to elicit further information from the child by asking additional questions, such as:

'Do you have an uncle?'
'What do you have to have to be a sister?'

The entire exchange was recorded by the interviewer on

a specially prepared interview sheet (1974:37). The authors report signs of boredom among the younger children "after the interviewer had gone through only four or five kin terms", in which case the child was allowed to rest and, if still unco-operative after his rest, was allowed to continue "a few days later" (1974:37).

The authors do not specify how many of the younger children were affected in this way. However, the fact that no complete protocols were obtained from children in the range 3;0 to 3;4 years suggests that the concentration of incomplete protocols was at the lower end of the age scale. The children who were allowed to rest could have done so for varying time-intervals, which would make comparisons between the results of different groups difficult. Also, the authors are vague as to the number of children who actually repeated the test after a few days. It would also be useful to know just how many days elapsed between test and re-test sessions : this, too, is not specified by the authors.

The study by Haviland and Clark cited above will now be contrasted with research carried out by Armbuster (1981). Armbuster carried out structured tests similar to those performed by Carol Chomsky (1969). He tested 28 children aged 5;5 to 6;4 and was careful to mention the details of his research method as well as his concern lest his method was incorrect. He displays his sensitivity in the test situation by placing the child to be tested at ease. He ensured that he was familiar with each of the children,

having seen them on an occasion prior to the test. He tested the children individually; each child came to be tested when ready to do so, and there was no compulsion as regards the test. He states (1981:31) that:

"The sessions lasted about 5 minutes, and were conducted in familiar surroundings".

Moreover, he mentions the care he took during the test itself to maintain a relaxed atmosphere which would be conducive to natural responses on the part of the child :

"When the child entered the room, he saw me sitting on the rug with the equipment. I asked him to sit down, and brought out Kathy. As did Chomsky, I tried to proceed 'fairly casually and in a conversational manner' (Chomsky, 1969, p.27). The goal was to find out about each child's linguistic knowledge. Encountered here is the classic problem of linguistic fieldwork. To perform systematic analysis, systematic data are needed. But the most interesting type of speech is natural speech, unmolded by inhibitions and affectations brought on by an artificial interview or experiment situation. So I tried hard to sound relaxed and informal, while keeping to my schedule of questions" (1981:31-2).

It is interesting that the Journal of Psycholinguistic Research, in which Armbuster's research is published, contains few articles in which a detailed account of the setting and the duration of the test(s) are mentioned. Just as in the case of the Journal of Child Language, most

details of method are omitted. In fact, Keil (1980) writing nine years after the Journal of Psycholinguistic Research had commenced publication, is one of the first researchers to give details of method. Keil appears, however, unaware of the problems inherent in long tests. In a test administered to groups of 1st, 3rd, 5th and 8th graders, he states baldly that testing sessions lasted from 30 minutes to 1 1/2 hours "depending on the skill and tractability of the child" (1980:225). Keil also does not expand on his statement (1980:225) that "standard uninformative prompts were used for the occasional unresponsive child". Just how many children were unresponsive is not clear; however, he does state that the test proved too long for four of the children to complete in one session and also that one first grader was dropped from the study because he could only manage a giggle as his response!

Some researchers writing in the Journal of Psycholinguistic Research do give interesting details regarding the physical setting of the test. Feagans (1980) mentions that the children sat at a low table in the experimental room and the experimenter sat across the table from the child. In similar vein, Barrie-Blackley (1973:161) relates that in the case of each subject (S) being tested,

"The S was seated in an adult-sized chair, at eye-level with the examiner, who was seated in a child-sized chair". The venue of the test was rather unusual in this case, being the hallway outside the child's classroom.

The researchers Hargrove and Panagos (1982) discuss an experimental session which was held "in a distraction-free room at a local college. This session lasted about 20 minutes to a half hour. A videotape camera, situated in front of the child at a distance of approximately 2 metres (6 feet) was used to record responses. Seated at a table next to the experimenter, the child was directed to act out the enactment sentences. It was noted that some of the sentences might sound 'funny' but that an effort should be made to act them out anyway" (1982:221).

Just as in other research cited previously, the presence of a videotape camera could constitute a distraction to the child being tested. Especially interesting here is the fact that the child is asked to act out responses to sentences in front of a camera, while seated. It may be that the children in question did not exaggerate their responses in this situation. However, the requirements of the test would seem to be conducive to some exaggeration, which would distort the results.

Further evidence of the effects of test duration on the performance of children is given by Brener (1983:240). In a 90-item comprehension test given to children of ages 2;8 to 5;7, it was decided that the youngest children should undergo a number of sessions on succeeding days. The older children were asked to do the test in one session, but it was broken up into three parts:

"To minimize the effects of fatigue there were two 10-

minute breaks during the session" (1983:240).

Hladik and Edwards (1984:324) also provide for the possibility of unreliable test data by filtering out certain sections thereof:

"In order to control for possible warm-up and fatigue factors, the first and last 5 minutes of each 30-minute sample were deleted from analysis..."

Although this process of data selection would seem to have merit, it is not common practice among researchers into child language. A large number of tests do have a pre-test session, but the data of the test itself is usually analysed as a whole. Further discussion therefore seems necessary on the issue of whether the so-called warm-up and fatigue factors should be allowed for, and, if so, to what degree.

Finally, research by Staley (1982) mentions a number of points which are germane to the question of testing children in experimental situations:

"At each of four age levels, 10 subjects were male and 10 were female. In order to reduce sex bias, exactly half the interviews were conducted by a male interviewer and half by a female interviewer"(1982:143). It has been argued that child subjects may possibly show higher levels of linguistic performance on tests where the interviewer is female. Alternatively, girl subjects may respond more favourably to female interviewers than boy subjects do. This could lead to generalisations such as "Girls are better at language than boys" or "Girls acquire language more rapidly than

boys" (my examples).

Staley (1982) goes on to describe the research:

"Each interview was preceded by a brief rapport-building conversation between interviewer and subject about a limited number of topics, usually about school, teachers, lunch, etc. Although it was impossible to standardize the content of these conversations, this was thought to be a necessary inclusion, because of the potential anxiety produced by the interview situation. Evidently some children did find the situation threatening, since four females at the 4-year-old age level refused to participate and had to be replaced" (1982:143).

The effects which factors like the sex of the interviewer and the anxiety level of the child have on the results of tests of child language development require to be researched in greater detail. It is only when more is known about factors such as these that controlled tests can be compared with confidence.

In this chapter, a number of articles on child language development have been examined. Most of these articles have appeared in the two main journals in the field, namely the Journal of Child Language and the Journal of Pyscholinguistic Research. The purpose of the chapter was to show that a number of factors are present in child language tests which render the results of such tests suspect, or, in any event, prevent the generalisation of the results to other contexts.

Briefly, the main factors involved are:

(1) A presence of a relatively strange interviewer.

(2) A test situation which is artificial.

(3) Potentially distracting apparatus, like tape-recorders, videotape cameras and notebooks.

(4) Fatigue and boredom induced by lengthy test sessions.

(5) A lack of uniformity in duration of tests, with respect to the age of the subjects.

(6) Anxiety experienced by child subjects.

(7) The absence of the child's mother, especially in the case of very young children.

(8) The effects of the gender of the interviewer on the responses of the child subjects.

(9) The period of familiarisation allowed for the child to get to know the interviewer.

In the articles which have been examined in this chapter, there is a distinct lack of uniformity of method. Standards of testing practice do not appear to have been formulated. This is a disappointing feature of the research published in the Journal of Child Language especially, since one of the founding aims of the JCL was to refine the methods being used in research into child language. It appears that researchers into child language in situations of experiments or tests actually have the worst of both worlds: in the first place their results are obtained in an artificial situation, while in the second, a lack of uniform controls and test practices makes it very difficult to

compare the results of one test with another.

Chapter 3

INFLUENCES OF AN ARTIFICIAL TEST SITUATION ON THE OUTCOMES OF TESTS OF SEMANTIC DEVELOPMENT

In the previous chapter, there was a critical discussion of the factors present in tests of LD which tend to render such tests invalid. The purpose of this chapter is to analyse a number of materials and procedures used in tests of LD. The main hypothesis herein is that the use of toys, models and contrived situations is inappropriate to the study of semantic development.

In tests of semantic development administered to children, it would appear to be important that, while preliminaries to the test may be light-hearted, the materials of the test itself should be realistic. The rationale behind this argument is that, if children are tested in situations which differ markedly from those of real life, then the results of the tests could only be used to reach conclusions about the meanings children attach to words in a context of fantasy.

It is important to examine the role of toys or models in language testing, particularly the perception that a child may have of the toy (model) and the extent to which adult testers and interviewers overtly recognise that toys and models are mere symbols. To illustrate this, one needs to examine the following example : a child playing with a toy in the form of a female human being refers to the toy, let us say, as <u>lady</u>. He does this because of the similarity

he perceives between the toy and an adult female. If on the other hand, suppose that he is given a toy elephant, but has never seen a real elephant, he would presumably not attach the same meaning to the word elephant, used to designate the toy, as would someone who had seen a real elephant.

In this chapter, a number of tests of semantic development will be discussed critically, with the foregoing distinction in mind. It will be argued that, in many cases, the make-believe nature of the test situation does not allow one to extrapolate test results to realistic situations.

Some games used in the administration of tests incorporate more make-believe than others. Where a game is merely a relaxed way of introducing a child into a situation in which language is used naturally, there might be merit in such an approach. However, when a game situation, including the materials and the models used in the game, bears little resemblance to that which the child is likely to encounter in his everyday life, then the test carried out may have questionable validity.

In this chapter there will be a discussion of tests of semantic development with reference to the reality of the test situation. The first of these is a test carried out by Donaldson and McGarrigle (1974) and is reported in an article entitled "Some clues to the nature of semantic development" in the first issue of the JCL. In this article the authors set out to study comprehension of the quantifiers all and more by children between the ages of

three and five. The authors introduce their study as follows:

"Consider the situation in which a statement has to be compared with a physical array and judged true or false in relation to that array. In such a situation, since statements do not ordinarily describe exhaustively, there are properties of the physical array which are irrelevant to the making of the judgment; that is, changes in these properties do not change the judgment. The properties which a given subject actually treats as irrelevant provide evidence about the meaning of the statement for that subject" (1974:185).

What the authors do not mention here is that statements which refer to a physical array are different from statements which refer to *models* of a physical array. In the case of statements which refer to a model, instead of to the entity which the model represents, the properties of the model itself may affect the interpretation (and hence the comprehension) of such statements by the listener. A person who hears, for example, the statement: <u>This elephant has big ears</u> arguably attaches a different interpretation to the statement depending on whether the reference is to a toy elephant, a photograph of an elephant or an actual elephant.

Donaldson and McGarrigle argue that, in statements of the form, "'All the Xs are in/on place Y'", the attributes of X and Y do not affect the truth of the utterance, apart from "the possibility of some uncertainty about the

boundaries of Y" (1974:185). They state that: "It would not matter in the least, for instance, whether the place Y was a container or an open surface - whether it was a box or a field or a city" (1974:185). The authors were interested to know why children under the age of five held the utterance: 'All the cars are in the garages' to be true in the situation where each garage contained a car and there was also a car "sitting in full view outside" (1974:185-6).

Such an interpretation was not found in the case of a statement like:

All the lids are on the pots

In the case of the latter, it was held to be true only if the numbers of lids and pots were equal.

The test material used by the researchers is described below, and an illustration of their material is also reproduced:

"The material consisted of nine toy cars and ten toy garages. Each garage could hold only one car. The garages were joined at their sides so as to make two separate garage structures, one with spaces for six cars and the other with spaces for four cars. The garages were without doors so that the cars were visible to the child even when garaged, and also without floors so that the garages could easily be placed over the cars or lifted away from them.

Display shelves were constructed so that one subset of the cars and its garage structure could be placed above the other. The top and bottom shelves were identical in

appearance. The garages could be positioned or removed by the experimenter from behind the shelves. A typical display is shown in Fig. 1.

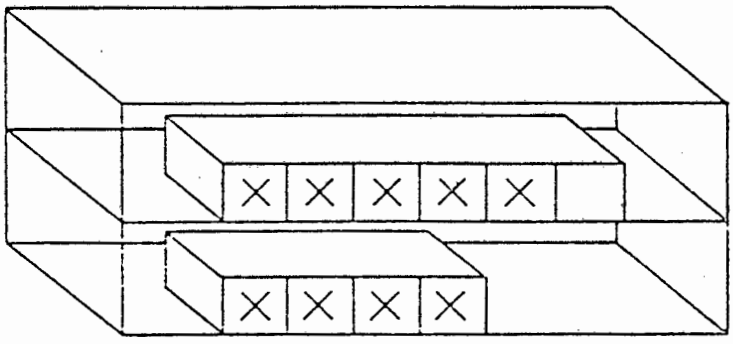

Fig. 1. A typical array, showing cars (Xs) and garages in position on the shelves" (1974:186-7).

Fig. 1

The experimenters do not stress the fact that the garage models are actually substantially different from real garages, in that they lack doors and floors. There is also no indication in the research that the experimenters sought to establish whether the child subjects could differentiate between a single model garage and a garage structure. In

fact, the latter ambiguity is evident from an extract from the authors' PROCEDURE section:

"(The experimenter puts both garages on the table.) What are these called? (indicating the garages) Right. These are the garages" (1974:187).

However, it is clear that by garages the experimenter is actually referring to garage structures. This is true both of his stage directions and of his actual words to the children being tested. It could be argued that this ambiguity alone vitiates the research because of the crucial importance of differentiating between a single garage and a garage structure (of four or six joined garages). After all, the subjects would presumably have already experienced the term garage being used to refer to a single structure and not to a row of joined garages.

But there are other problems with this research study. The fact that the garages are moved away from the cars (and not, as a rule, vice versa) lends an unreal flavour to the whole situation. Not only is the child being asked to interpret statements which contain quantifiers,but he is also required to do so with models of physical objects whose resemblance to the real thing is weak. Also, the fact that garages are not only in rows, but tiered (on shelves) is a further source of potential confusion for the child. He may not be familiar with such multiple garage arrangements, especially in three dimensions! An element of spatial perception testing, in addition to pure linguistic testing,

is being introduced.

For the crux of the test, the procedure was as follows:

"For all of the judgments the child was faced with a line of five cars on one shelf and four on the other. The cars were arranged so that the garages could be positioned without disturbing the cars. The four-space garage, when present, was always full; the six-space garage always placed in vertical one-to-one correspondence which began on the left-hand side of the display, so that the extra one car (and one garage space) always appeared on the right, as viewed by the subject. The child was asked six questions, four involving all and two involving more. For the more judgments the child was asked:

Are there more cars on this shelf or more cars on this shelf?

with the experimenter pointing appropriately. The same question was then repeated, the garages having been either removed or placed in position. For the all questions the child was asked:

Are all the cars on this shelf?

for each shelf successively, with the experimenter again pointing to the intended referents. The two all questions were then repeated, the garages having been either removed or placed in position. The child completed all the judgments for one type of question (i.e. either all or more) with garages both present and absent, before receiving questions of the second type" (1974:188).

It is interesting to note, in *this* description, that the writers refer to "one garage space". Nowhere in their description of their preliminary conversations with the child subjects do they refer to a "garage space", presumably because this term is a complicated one for a child to understand. When they say to the children: "Right. These are the garages" (187) it is not clear whether they are pointing to *individual* garages (i.e. garage spaces) or garage *structures*. It would appear that the term garages may be ambiguous to the children: it could be taken to mean either "more than one garage (space)" or "more than one garage structure". As pointed out earlier, this lack of clarity of meaning weakens the test substantially.

Another problem arises with the phrasing of the question:

Are there more cars on this shelf or more cars on this shelf?

The purely linguistic "correct" answer is this shelf so that the child would have to gesture appropriately as well. However, since the phrase this shelf may be used first to refer to the shelf with fewer cars (the top shelf), it is possible that the child could point, incorrectly, to the top shelf partly because *its label* happens to be correct. This problem could be overcome by phrasing the question thus:

Which shelf has more cars? or even

Which shelf has more cars on it?

Since the child is likely to use a gesture in order to

answer the original question correctly, he would also be able to do so in the case of the suggested amended questions. He would not, however, be able to latch on to a linguistic 'clue' if the latter two questions were used.

Another point that the writers do not mention, albeit a minor one, is that their more questions actually contain two "mores". This is rather awkward stylistically and would presumably be defended by the authors on the grounds that it is more neutral than saying:

Are there more cars on this shelf?

and then pointing to one of the shelves. If one were to ask the latter question and point to the shelf with five cars, it could be construed as giving the child a clue as to the correct answer. However, the fact that the test question contains a 'double' more is significant. It means that a slightly different test from that originally envisaged by the writers is being performed. It is also possible that some children could find the question confusing. In normal adult usage part of this question would be deleted:

Are there more cars on this shelf or on this shelf?

In their analysis of the results of this test for the questions involving more, the authors found that 21 children answered correctly and consistently, and five children consistently gave incorrect answers. The other 14 children: "changed their choice of subset when the garages were introduced or removed. Thirteen of these correctly chose the larger subset in the absence of the garages, but chose

the smaller subset (i.e. the full garage) when the garages were in position around the cars" (1974:188).

On average, the children who answered incorrectly or inconsistently were slightly younger (mean age 4;1) than those who answered correctly (mean age 4;4).

Regarding the four all questions, only six children out of the 40 tested answered the whole battery correctly. However, the authors do not give a full analysis of which groups of children who received the all questions before the more questions answered correctly, and vice versa. In this respect, the test design appears faulty, since it is reasonable to assume that children who received the more questions before the all questions could have interpreted the question:

Are all the cars on the shelf?

as:

Are all the cars (that were on this shelf in the first part of the test) on this shelf?

The authors were apparently aware that the children could be interpreting in this way, since they refer to three children who, they think, were interpreting "all as being about more" (1974:190) where the children concerned had received the more questions before the all ones.

However, the analysis of the results does not appear detailed enough, since there is no break-down of responses of the children who received the more and all questions in reverse order. The importance of the order of receiving the

all and more questions appears to have been either overlooked or minimised by the authors.

In their conclusions, the authors mention three kinds of rules which the children being tested appear to use. These are (1974:193): LEXICAL RULES, which are "not finely specified"; SYNTACTIC RULES, which "impose few constraints by comparison with the constraints imposed by syntax on an adult interpretation" and LOCAL RULES which "are not linguistic rules in the narrow sense though they interact with lexical and syntactic rules in ways that are critical for the interpretation of utterances" (1974:193).

The authors give the following as examples of LEXICAL and SYNTACTIC RULES used by children (1974:190):

LEXICAL RULES

"Thus more, in phrases like more cars or more water, may at a given point in development indicate merely that a difference is being referred to and, in some general and ill-defined way, that it is a difference in magnitude (and not, for instance, in colour) that is at issue."

SYNTACTIC RULES

"It is, however, hard to say much about them at present except that they, too, impose few constraints .... Thus, 'All the Xs are in Y' can be treated as equivalent to 'All the Ys have Xs in them'; that is, the question of which noun is quantified, a question that is clearly crucial for an adult interpretation, may be disregarded (Donaldson and Lloyd in press)".

The authors' description of LOCAL RULES is essentially that they "determine those features of the referent which will be selected as criteria for the assigning of truth values when the linguistic rules leave the matter vague. They determine that an utterance shall be interpreted now in one way, now in another - that there shall be held to be more cars in the garage structure which is full although shorter, yet on the other hand more cars in the longer row when the garages are not there" (1974:193-4). Their justification for postulating LOCAL RULES is that they are an

"alternative to supposing that for young children 'the meaning' of an utterance frequently changes. If we do not introduce some such notion we are forced, by evidence of the kind presented here, to conclude that, for the children, a complex set of alternative meanings inheres in the language itself" (1974:194) and the task would then be to explain systematic preference for one meaning in a particular instance.

It would appear that the strength of this argument is dependent on the following assumptions:

(a) children can be relied on to answer test questions honestly;

(b) children perceive the test in a sufficiently realistic way, in that the use of toys and models does not lend too artificial an aura to the test;

(c) children attach the same meanings to key words like "shelf" and "garage" as adults do;

(d) random patterns of incorrect answers are not present to a significant extent, i.e. the

justifications which children give for incorrect answers are not <u>in themselves</u> suspect;

(e) the questions asked of and instructions given to the children are unambiguous.

As has been already argued, it is the contention of this thesis that, especially with regard to assumptions (b),(c),(d) and (e), the authors are on dangerous ground. Asumption (a) is difficult to test; however, some children may alter their answers for frivolous reasons or because they have incorrect perceptions of the purpose of the test. An intelligent child may, for example, fail to see the point of the particular questions in this test, and change an answer which he knows is correct to an incorrect one because he may think that there is more to the questions than meets the eye.

However, the overriding criticism of this test is that it contains elements which are, to a large extent, removed from reality. Since truth values of utterances depend heavily on the realistic perception of those doing the uttering, it would appear in this sense, that this type of test is doomed from the beginning.

In contrast to the one just discussed, a far more acceptable test of semantic development is that performed by Andersen (1975). Children were asked to name 25 different drinking vessels, most of which would be termed either <u>cups</u> or <u>glasses</u> in standard adult English. Most of the items were familiar to the children, and those that were not, were similar enough to familiar vessels for children to be able

to make a realistic attempt at naming them. Having named the various vessels, the children were then asked to sort them into two groups: cups and glasses. This exercise, too, was realistic since children routinely perform sorting of their toys or other possessions. Finally, the children were asked to define the terms cup and glass. This was done along the lines of the following:

"Suppose you had a friend from another country and that friend didn't speak English very well. And one day he/she said to you. 'My mother told me to go to the store and buy some cups, but I'm not sure what a cup is. Can you tell me what it is/what it looks like? What would you tell him/her?" (1975:85).

Even though this is, by the admission of the experimenter, an "imaginary situation" (1975:85), it is nevertheless a plausible one. A minor criticism might be that, in a real situation, the urgency of the need to convey information of the type required would spur the child to provide a more complete or more accurate description than he or she would in the test situation.

However, the test as a whole comprises unambiguous instructions, although the last one, in which the children were asked to choose the best exemplar may have resulted in some confusion for the younger children who may not quite have understood this instruction:

"Lastly, each child was asked to choose the best examplar for each category from the array on the table. The

instructions were to choose 'the best example of' or to choose 'the one that looks most like each class sorted'" (1975:85).

It is uncertain whether children as young as three years (the age of the youngest group tested) would understand the phrase best example of, although the alternative explanation on the grounds of similarity in appearance might be understood much better. Even here, however, children may be confused when asked to point out which of a set looks most like a glass, especially when the items in question bear a close resemblance to one another.

All points of method considered, this test is far more satisfactory than that of Donaldson and McGarrigle (1974) described earlier in this chapter. The approximate duration of each session (each child being tested individually) was fifteen minutes. This is also pleasing, since in this relatively short time the children would be unlikely to become bored or suffer a loss of concentration.

Charney (1979) discusses a test of comprehension of the deictics here and there by children between the ages of 2;6 and 3;6. She describes her procedure as follows:

"The experimenter and child sat on the floor in one of the three arrangements shown in Fig. 1. Two moving toys at a time were used: an airplane and a train, or a car and a bus. The experimenter and child both participated in sending them across the floor to their appropriate places, with the child being instructed, with accompanying points,

to send the vehicle 'near me', 'by the wall', etc. Instructions containing spatial deictic words were, of course, avoided. After the toys were in place, the experimenter said, for example:

See the airplane? (waited for the child to look at it).

See the train? (waited for the child to look at it).

Which one is over here/there?

The child was then expected to point to, name, or touch the correct object. The experimenter always looked at the child and never gestured. One exception was if the child did not respond to, for example, 'See the airplane'. In such a case, the experimenter would point out the airplane to the child, make sure he looked at it, and then resume the procedure, again looking only at the child.

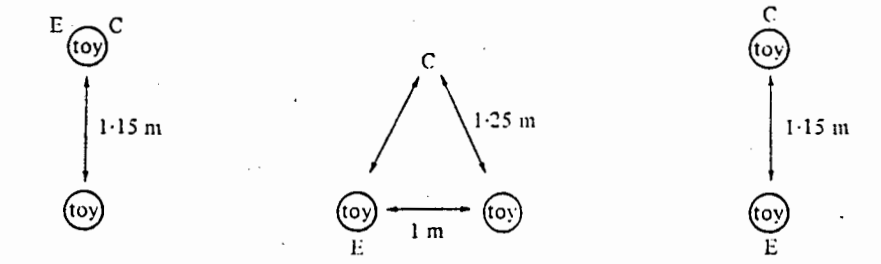

Fig. 1 Position of E (experimenter), C (Child) and toys in the three perspective conditions. Approximate distances (in metres) are indicated."

(1979:71-2)

Fig. 2 (Fig. 1 in Charney, 1979)

The artificiality of the experimental situation gives rise to a number of flaws in this test. First, the fact that the experimenter has to ensure that the child is paying attention (or obeying instructions) by looking at the appropriate object when asked to do so, raises some doubt as to whether the child's responses to the key question thereafter, viz.

Which one is over here/there?

are reliable. The child may not fully distinguish the car from the bus, for instance, and his lexical response (if any) may be incorrectly stated, i.e. he may mean bus but say car. The fact that only vehicles (or means of transport) are used here, could give rise to errors of overextension as well as of deixis. On the other hand, whimsical answers are also possible (the children tested being very young, in the age range 2;6 to 3;6).

The second flaw in this test is the situation termed

the neutral perspective. Although the toys are equidistant from the child, both toys are closer to the speaker (the experimenter) than to the child. A more neutral arrangement would have been to have the child, experimenter and the toy (further from the experimenter) situated at the vertices of an equilateral triangle, thus:

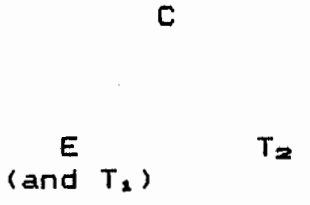

Fig. 3

In the above figure, T_1 and T_2 represent the toys. Another minor flaw, or discrepancy, is that the toys are actually slightly further from the child in the neutral perspective (1,25m) as opposed to the distances in the other cases (1,15m). Although this is not a serious difference, the child being tested may be influenced by the slightly longer distance involved in the "neutral" perspective. The use of the word "neutral" here is also a little odd, in that it is not, strictly speaking, a neutral perspective. Before discussing what is perhaps potentially the major flaw in the experimental design, it is useful to examine the test results for the entire group. For analysis purposes, the author (1979:73) has split these into two sub-groups, by age, with 25 children in each sub-group:

TABLE 1. *Percentage correct in the three perspective conditions*

	Same			Neutral			Opposite			
Age	*here*	*there*	Total	*here*	*there*	Total	*here*	*there*	Total	Total
2;6–3;0	63·0	86·0	74·5	67·0	80·0	73·5	73·0	64·0	68·5	72·0
3;0–3;6	83·0	89·0	86·0	73·0	91·0	82·0	89·0	64·0	76·5	82·0
Total	73·0	87·5	80·3	70·0	85·5	77·8	81·0	64·0	72·5	77·0

It is interesting to note that, for both age sub-groups the percentage correct for here is less than that for there, in the case of the condition, "same perspective". The difference (in this perspective condition) in percentages between the younger and older sub-groups for "here" is 20,0. This seems strange since one would have thought that the use of here, in this condition, would be correct to a large degree for both groups. Certainly, here should not be more difficult than there, on an intuitive analysis of these perspectives. However, if one examines the key questions again, one reads these as :

Which one is over here? and

Which one is over there?

The use of over in conjunction with here is a little odd to say the least, if this is indeed what the experimenter did. Admittedly, the children involved spoke an American dialect of English, in which over may be more "neutral" than in other parts of the English-speaking world. However, the use of over here is odd. If both E and C share the same perspective, then it should suffice to say, simply, "here". The children being tested, especially

the younger ones, could well have been confused by the use of this phrase. It seems more natural to use the phrase over here where speaker and hearer are in different places, not when they share the same perspective.

The use of the expression over here appears to have arisen because Charney adapted the procedure used by de Villiers and de Villiers (1972) to a different set of circumstances without fully considering a crucial distinction. Charney uses over here when both child and experimenter are in the same place, whereas de Villiers and de Villiers do not. To quote Charney:

"In the de Villiers study the experimenter and child always sat on opposite sides of a short wall. The child could successfully find a candy hidden under a cup by following the experimenter's clues of 'the M & M is on this/that side of the wall' and 'The M & M is over here/there'"(1979:70). Clearly, over here is used in a crucially different way in the de Villiers study.

Kuczaj and Daly (1979) describe a "quasi-experimental study" (563) on the subject of hypothetical reference in the speech of young children. The authors regard the development of the child's ability to indulge in hypothetical reference as a "subtype of the child's developing ability to refer to non-present objects and occurrences" (1979:564). In their introduction, the authors are careful to classify "non-present happenings" into six categories, each category representing the degree of belief

held by the speaker in the event either having occurred, or being likely to occur in the future.

This study comes up against one of the central problems experienced by researchers into semantic development : that of interpreting the responses of children to adults' questions. But here the problem is compounded, because this study involves children's responses to <u>hypothetical</u> questions. For very young children it is difficult to say where the line between fact and fantasy lies : it is precisely this distinction which is crucial to the interpretation of children's utterances. For example, the authors give the following as an example of an "irrelevant response" to a hypothetical question:

" Adult

What if you didn't grow up? What if you stopped growing up and grew out sideways and just got wider and wider?

Child

I'm gonna be hey-dittle-dittle in the moon. I wanta be hey-dittle-dittle the cow. I'm gonna be hey-dittle-dittle in the sun (A.K. 2;10)"

(1979:568)

The authors do not say why they regard this (2 year and 10 month old) child's response as irrelevant. The experimenter's question is (in a sense) nonsensical, since children presumably believe that they <u>will</u> grow up (both literally upwards and maturationally). It is not surprising, then, for a young child to respond in terms of what (to an adult) appears sheer fantasy. After all, if he is not going to grow up normally, into a normal adult, surely his desire to become a nursery rhyme character is,

for him, the choice of a possible alternative! If children can't grow up into normal people, then why not into animals? For a child as young as 2;10, the reality or otherwise of nursery rhyme characters is hard to determine. This makes the classification of this response as irrelevant, difficult to understand. It is not as if the child had uttered a nonsense syllable in reply; it does seem that the child has made an attempt to respond to the demands of the question.

Kuczaj and Daly (op. cit.) began their testing procedure by asking each child individually to tell a story about a lion. They describe their (i.e. the experimenters') behaviour as follows:

"During this phase, the experimenter avoided using hypothetical terms, instead using prompts such as What happened next? and and then what happened? to encourage the child to continue with his story. Following this, the experimenter read one of a set of three very short stories to the child, and then immediately asked the child factual, future hypothetical and past hypothetical questions about the story. This procedure was then repeated for the remaining two stories" (1979:567).

In a subsequent analysis of children's hypothetical reference, the authors refer to the "non-hypothetical reference story (about lion)" (1979:571) and contrast this with the "hypothetical reference stories" which they (the experimenters) told. This distinction appears odd since all the children (presumably) invented their story about a

lion, surely this, too, ought to be classified as hypothetical reference. Surely the difficulty here is to separate fact from fantasy in the perceptions of the children being tested. Once a story is being related, both speak and listener are involved in the context of fantasy. A child who says Then I'm a cow as opposed to Then I would be a cow cannot really be said to be meaning something different from what is entailed by the second sentence. The authors have to contend with this problem in attempting to classify childrens' utterances (1979:572-3) as being either internally consistent or inconsistent hypothetical reference sequences.

They do, however, acknowledge the tentativeness of their research in their discussion of the data, by saying that they are offering "a speculative account for each of four developmental patterns" (1979:575). In this connection, the authors' fourth-mentioned developmental pattern, viz. "children's higher degree of accuracy in self-initiated than in other-initiated hypothetical reference" (1979:575) is interesting. This suggests that a child finds it easier to construct a sequence of hypothetical references than to comprehend a sequence constructed by an adult (1979:578). Many tests of semantic development (including this one) involve children in first comprehending the cognitive framework of the test and then making appropriate responses within that framework. However, if the test appears to be too much like a game, then a child may respond

capriciously. It may not be that he is lacking in the specific area of semantic development being tested; it may simply be that he is overwhelmed by the test situation as a whole and therefore adopts a strategy which avoids the real issues. In short, the child, in an artificial test situation, manipulated by an experimenter whose aims and motives are beyond the child's understanding, may not be a reliable subject, if indeed he is able to be a subject at all.

Corrigan and Odya-Weis (1985) carried out a study of the comprehension of semantic relations by two-year-olds, using tokens (a red felt triangle and a green felt square) to represent actors and patients in sentences with both subjects and objects. Both actors and patients could be animate or inanimate. In a preliminary training session, the children were shown a number of pictures depicting actions, e.g. a boy pulling a boat or a bike pulling a dog. The experimenter would then place one of the tokens on the agent and the other on the patient, using one token consistently as agent and the other consistently as patient, for each child. In all, 48 children were tested of whom half were trained to associate the red token with the actor and half to associate the green token with the actor. The voice of the sentences was also varied: "In all presentations, the sentence form was varied so that half were in the active voice and half in the passive voice" (1985:52). Once the child had seen the experimenter placing

the tokens a number of times, he (the child) was asked to take over the placing until "the child correctly placed both tokens for 5 of 6 consecutive sentences or for 5 in a row" (1985:52).

The child was then directed to the body of the test, which involved the placing of tokens on 12 pictures: "three new sentences for each sentence type (AI, AA, IA and II) were presented, once each with no corrective feedback given. Both tokens were available, so that the child had to choose which tokens were appropriate. Children were praised verbally or given a star or sticker when they placed the tokens on the pictures, even if their responses were not correct" (1985:52-3).

In this context, the symbols A and I appearing in the sentence types represent Animate and Inanimate respectively, e.g. AI stands for a sentence in which the actor is animate and the patient inanimate.

Before discussing the test results, some comments on the authors' method are required. Praise is a very strong reinforcing action with children; one wonders, therefore, why an incorrect placing of tokens by a child was rewarded. It seems likely that this could lead to the child forming a mistake pattern, and being praised for each successive mistake! It is not unlikely that two-year old children, who have only recently been "trained" to distinguish between actors and patients by a process of association in short-term memory, could just as easily <u>reverse</u> their response

pattern if they were praised for an incorrect response. Also, since the child is accustomed to experiencing real-life actions of kicking, pulling and pushing (all of which tend to be of relatively short duration), he may not necessarily interpret the static "actions" depicted by pictures in quite the same way as he would a real action.

Even though the authors were careful to avoid a right-left or large-small bias (1985:51) in the actors and patients appearing in the pictures, the fact that both actor and patient are static in the pictures is at odds with what normally obtains in a real situation, where the actor is seen to "move first", as it were. What the authors are really testing is the child's comprehension of actor and patient in static pictures, as opposed to the comprehension of this semantic relation in a natural, dynamic occurrence. The difference is subtle; yet it is important: semantic relations are broader than those formed by a study of pictures alone.

The fact that children's semantic development is often tested with the aid of pictures and toys, in a more or less constrained experimental situation, makes it difficult to generalise the results of these tests. They may, through their artificial nature, be seriously lacking in validity. Unlike adults, who possess advanced metalinguistic ability, children, who are not able to read and thus reflect on sentence structure to the same extent as their elders, are problematic subjects in tests of this nature. Serious

thought needs to be given to finding methods of probing or assessing (rather than testing) children's semantic development under less stilted and restricted conditions.

Fromberg (1976) illustrates a language-learning game which appears to have some merit since it employs objects which the child actually sees and is able to touch. In her "grab-bag" game, the teacher models descriptions of objects which are at first hidden inside a bag, but are later revealed to the onlooker:

> "I feel something cold
> I feel something wet
> I feel something cold and wet but not greasy.
> It's ice! "
> (1976:254)

This procedure is then repeated with objects like cane and sandpaper. The children are then asked to play the game: the fact that real sensations are being described, albeit in a game context, is a positive feature of this research. However, if children are asked to describe pictures of objects of which they may not have "hands-on" experience, their descriptions will not necessarily be as reliable. They may simply describe what they think are the correct attributes of the object depicted.

MacWhinney (1983) describes a test involving a minature linguistic system (MLS) consisting of "eight object names" and "four locative affixes" (1983:471). Children had to place toy animals in various locations relative to the objects. The animal models used were: "frogs, dinosaurs, lions, monkeys, bears, dragons, trolls and robots"

(1983:473). The choice of animals is rather strange, especially those of dinosaur, dragon, troll and robot. It would certainly confound a biologist to be told that a robot was a type of animal! Certainly, this choice is not quite in keeping with the author's stated aim of maximising "linguistic naturalness and referentiality" (1983:467). The game takes on an even less realistic note when the author describes his procedure:

"For example, the four dragons were called Puff, Harry, Feather and Diane. The child was then told that the dragons (or the frogs or some other animal family) wanted to visit one of the four 'hotels'. For example, the dragons may have been interested in visiting the 'nak'. Thus, Puff may want to go naksib, Harry might want to go renaken ....."(1983:473-4). The author defines nak to mean cube and the affixes -sib and ra- -em to mean "in" and "behind" respectively. (Incidentally the word renaken in MacWhinney's article should be spelt ranakem).

Despite the fact that, in Hungarian and other natural languages, noun-plus-affix structures do exist (1983:471), the "naturalness" of the language alone is not sufficient to guarantee a natural language situation. Since extinct and incorrectly defined animals are used as models, the situation takes on an artificial aspect. Although nonsense words may not have the same effect on all children undergoing the test, some children may find them confusing. The combination of nonsense words and far-fetched exemplars

of animals is also unfortunate : it lends an unnecessarily artificial flavour to the whole experiment.

In this chapter, a number of research articles have been cited and discussed to illustrate the point that semantic tests administered to children often lack a realistic context. It would appear that some researchers do not pay enough attention to making their test situations as life-like as possible. Because these tests are, to a greater or lesser extent, artificial in nature, the conclusions reached therein are open to question. In order to overcome the objections which have been raised in this chapter, and to avoid pitfalls in future tests, the following basic guidelines should be adopted:

1. Test materials should be realistic.
2. Models should bear a close resemblance to the objects which they represent, both in form and in function.
3. Exemplars of categories should be familiar; for example, if animals are being used, then exotic animals should not appear in the test.
4. Models and artifacts should be avoided, and the real thing used wherever possible.
5. Pictures which distort proportion should be avoided.
6. There should be no ambiguity in the description of the test procedure.
7. The language used to address the children being tested should be clear and concise.
8. As in all tests of language development, the time should be kept to a minimum, in order to prevent boredom and fatigue on the part of the child subjects.

There are clearly some researchers who use games

skilfully in tests of language development. However, the connection between the game and the real situation of everyday language use needs to be understood very clearly by the researcher.

Chapter 4

A CRITICAL DISCUSSION OF THE LANGUAGE OCCURRING IN TEST ITEMS IN STUDIES OF SEMANTIC DEVELOPMENT

In Chapters Two and Three of this thesis, there was a general discussion of the artificial nature of language development test situations and the inappropriateness of various test props and materials. The language used in the test items of a LD experiment is important for the obvious reason that clarity of meaning is a precondition for obtaining valid responses. In this chapter, two issues in research methods used in semantic development studies will be discussed, both of which involve the type of language used in test items.

The first issue is the use of "nonsense words" or "nonsense syllables" in tests of semantic development. Nonsense words (syllables) were given prominence in tests of LD by Berko's (1958) research into syntactic development. Berko's test involved the use of a number of nonsense syllables (NS) e.g. wug. In one section of the test, subjects were shown a picture of an object resembling a bird and were told : "This is a wug". They were subsequently shown a picture of two of these objects and were asked to name them. The required response was : "There are two wugs". Berko had set out to test whether the subjects had mastered the use of the English plural affix -s. A nonsense syllable was chosen to obviate the objection that the word

bird (and its plural, birds) were aleady familiar to the subjects. It is interesting to note that Berko's research concentrated on *syntactic* development. It was published in the year following the appearance of Chomsky's Syntactic Structures in 1957.

Berko tested a group of children in an experimental setting, using NS as well as 'real' words. Interestingly enough, the syntactic inflections which children gave to 'real' words were more often correct than those they gave to NS. Her research set a precedent for the use of NS in tests of language development. It is thus not surprising that a subsequent researcher, Harris, should make use of NS in a test of semantic development.

In his article "Inferences and semantic development", Harris (1975) made use of NS in order to test whether "the nominal predication of an unknown word by a superordinate term enables young children to make appropriate inferences concerning its attributes" (143). Harris gives two reasons for the use of NS in his research : "First, because they could obviously not be ascribed attributes except on an inferential basis. This would not necessarily be true if the subject were questioned about a familiar hyponym. Secondly, it was hoped to provide an approximation to normal acquisition processes in which a new word is defined by inclusion" (1975:143).

In this chapter the three experiments described by Harris will be discussed, with particular reference to the

use of NS. After this discussion, there will be an assessment of the usefulness of NS in research into semantic development. An important issue is whether NS are suitable instruments in tests of semantic development, or whether they are more suited to tests of syntactic development. In his research, Harris used children aged from 4;6 to 7;4. In the first experiment 16 children took part, in the second experiment there were 32 children and in the third, there were 24. Harris does not describe the duration or setting of the experiment, nor does he state whether some of the children took part in more than one experiment.

Harris's first experiment

This was divided into two parts. In the first part, "the children were questioned about the attributes of one of four entities - man, bird, aeroplane and house. Three questions were posed:

(i) Does a ---- eat food?
(ii) Does a ---- have wings?
(iii) Is a ---- alive?

The same three questions were then asked of the next entity, and so forth. Answers were recorded as yes or no. The second stage of the experiment consisted of the same set of three questions posed in relation to four NS - mib, lak, wug, dop. An NS was paired with an entity by simple nominal predication. For example, the child would be told a 'mib' is a man" (1975:144).

Harris states that the experimenter checked whether the information had been understood by asking the child "and

what is a mib?" If the subject replied correctly, the experimenter proceeded with questions (i)-(iii) above (1975:144). However, in some cases subjects "failed to identify a mib correctly, in which case the definition and preliminary checks were repeated" (1975:144). Harris does not elaborate on these failures to understand the NS, but they do appear to be significant. If nothing else, they appear to indicate that the subjects were having difficulty in defining a NS, or at least in equating it with a familiar lexical item. The potential confusion caused by the use of NS will be discussed in more detail later in this chapter.

Harris explains that the pairing of the four superordinate terms (which he refers to as entities) with the four NS was "systematically varied across children to rule out the effects of onomatopoeia" (1975:144). Children were then asked questions about the attributes of the hypothetical object referred to by each NS. The assumption here was that the children would be able to infer these attributes from those of the entity (i.e. superordinate term) to which the NS had been linked. Harris explains further that "each question was designed to elicit two affirmatives and two negatives across the four entities" (1975:144). This design, one must assume, came about in order to minimise the effects of patterns of guessing by the children e.g. a pattern in which a child answered affirmatively to every question.

Results of the first experiment

Harris found that almost all of the 16 children tested gave the correct attributes for the NS with respect to questions (i) and (ii). However, six subjects, in the case of question (iii) had difficulty in deciding whether the NS associated with the lexical item aeroplane is animate. Furthermore these six children, together with another three, answered in the affirmative to the question Is an aeroplane alive? Harris does not investigate the question of an aeroplane's animacy further. This is unfortunate, since it would have been an interesting line of study.

In retrospect, it is difficult to determine whether the use of NS in the first test is justified. Bearing in mind that Harris had taken great pains to equate each NS with a corresponding lexical item, it would seem reasonable to assume that the subjects treated each NS as a code name or strange synonym for the entity with which it had been linked, rather than as a hyponym. Therefore, the main criticism of Harris's first test is that it does not clearly achieve the goal of establishing that the child subjects understand and apply the hyponymy relation at all. The problem of the animacy feature of aeroplane is also left unresolved. In fact, Harris omits aeroplane (as well as house) from his second test without giving reasons for these omissions.

Harris's second experiment

In this test, Harris used four superordinate terms and four hyponyms, one for each superordinate. The lexical items used were bird (robin); man (busdriver); flower (rose) and drink (milk). As in the first test, Harris refers to the superordinate terms as "entities".

The second test was designed to investigate whether the child subjects could show evidence of recognising the superordinate terms as such. In his procedure, Harris used the same questions as in the first test, but "an additional question was also posed, e.g. Is the robin a bird? Is the busdriver a man?,etc." (1975:148).

After this, each "entity" was again paired with a NS, and the four questions were asked with reference to the NS. The fourth question in this case took the form : "Is a mib a robin?" (1975:148). There is a subtle yet important difference in the wording of the questions used by Harris in this test. In the questions involving the real hyponyms, he uses the definite article the, as in Is the robin a bird? However, in the questions containing the NS, he uses the indefinite article a, as, for example, in Is a mib a robin?

In English, the article the, being specific, usually precedes an exemplar whereas a is more often found before a category. It would therefore seem that, by using the syntactic framework Is the X a Y?, the implication is made that X is a hyponym of Y. This framework acts as a subtle

clue to the listener. Having used this framework in the 'real-word' questions, Harris switches to the form found in Is a mib a robin? in his subsequent NS questions.

Harris appears to be inconsistent in his use of syntax here. The NS mib, which had been equated with the entity bird in the earlier part of the research, but which Harris intends to take the place of a hyponym of bird, is now preceded by the indefinite article a. However, being a placeholder of an exemplar, it should be preceded by the. A similar comment applies to the use of a before robin in this NS question : its use suggests that robin is not an exemplar, but a category. By rights, then, Harris should have used the following wording :

Is the mib the robin? A negative answer to the latter question, taken in conjunction with an affirmative answer to Is the robin a bird? would be a better indicator of the child subject's possession of a superordinate category.

However, the use of NS bedevils Harris's research : simply because a NS does not refer to a real object, and is therefore a vague form of reference, it is odd to allow it to be preceded by the article the. The very use of NS thus places this research on a shaky basis.

Results of the second experiment

Harris obtained the result that "for the three entities, bird, man and flower, subjects were as likely to match as mismatch. Thus knowing that a mib was a bird, a child was as likely as not to agree that a mib was a robin"

(1975:148). This finding is not surprising in view of the potentially confusing use of a and the in the test questions. In fact, a seemingly incorrect answer, i.e. that a mib is a robin, might have come about through the postulation of a second tier of classification on the part of a subject, viz. that a mib is (a type of) robin and that a robin is (a type of) bird.

However, Harris mentions that a number of children justified their answers in sentences which indicated that they do "possess a genuine superordinate category". Examples of such sentences were :

"Some flowers aren't roses" and

"A few birds are robins"

(1975:148).

With explanations such as these, it appears as if the test design could have been different; rather than using NS, Harris could have spent more time on attempting to elicit this type of utterance from the subjects.

Harris points out that, in the case of the NS dop, there was a general tendency to mismatch this with milk. Most of the children answered that these two were equivalent. Interestingly, Harris omits the form of the actual question here. If the form Is a dop a milk? was used, it would have been ungrammatical, and any other form would have been inconsistent with that used in Is a mib a robin? Harris does not elaborate on the reasons for the different results in the case of dop and milk, other than to

state that some children justified their mismatch by referring to some quality of milk (e.g. its colour). It is a further flaw of Harris's research that he selected a superordinate term which can function as a verb as well as a noun, i.e. drink. For many children, the use of drink as a superordinate may not be very familiar. An adult might be more likely to ask a child

Would you like something to drink? than to ask

Would you like a drink?

This is a minor point, yet it may contribute to Harris's results in the case of milk.

Harris's third experiment

In his third experiment, Harris paired a NS "with a superordinate category plus an attribute, e.g. A 'mib' is a red bird" (1975:149). This experiment sought to assess the child's "sensitivity to attributes which distinguish subordinates from each other" (1975:149). The finding was that the addition of an attribute did not impair the children's ability to infer common attributes. For example, whether a bird was red or white, it was still thought to have wings. On the other hand, children denied that a "NS defined as a white bird was a robin or that a NS defined as a red drink was milk" (1975:149).

Interestingly, a large number of children would not agree that the NS defined as red bird was, in fact, a robin. Harris ascribes this to the children's perception of a robin as being only partly red. Many children thought that robins

aren't red because they have brown feathers. This is a small point, but it does show that children are able to perceive subtle differences in meaning, differences which may be glossed over by adults. In designing tests of semantic development, experimenters need to be careful that their descriptions of referents to be used in the tests are extremely accurate.

However, the children's reluctance to agree that a NS defined as a red bird was a robin may also be taken to mean that they are confused by the use of a NS. When the NS was originally associated with bird, it may have been still relatively vague in its reference. On the other hand, its association with a specific animal, a robin, may be less easy for the subject to accept. This is another difficulty underlying the use of NS in Harris's research. Seen as a whole, the use of NS by Harris does not appear to have been a profitable exercise. He appears to have overlooked some basic differences between using NS in a test of semantic development as against one of syntactic development. The first of these is that semantics is more 'sensitive' to nonsense than syntax is. A worthwhile question to be considered is : should one test semantic development by using non-words, i.e. nonsense syllables?

Another important consideration is the purported use of NS to avoid introducing a 'clue' to the subjects of the test. In Berko's test wug was used instead of the lexical item bird. This was because of the probability that the

children being tested had already heard the plural form birds. Harris uses NS "because they could obviously not be ascribed attributes except on an inferential basis" (1975:143). However, by using a less familiar, yet real, hyponym, Harris would have been likely to have achieved the same aim. Since he was testing British children, he could for example have used an unfamiliar bird like a kiwi as a hyponym. In fact, to ensure that a word like kiwi is unfamiliar to the children, one could ask appropriate questions in a pre-test, such as What is a kiwi? In this way, an experimenter could compile a list of words which were unfamiliar to the child subjects before commencing the test proper.

His second reason for using a NS was "to provide an approximation to normal acquisition processes in which a new word is defined by inclusion". By using an unfamiliar hyponym, selected by means of the method described in the last paragraph, an experimenter could meet this requirement as well. The use of a NS in order to achieve an approximation to normal acquisition processes is odd. Normal semantic development precludes the acquisition of nonsense syllables - at least as hyponyms of real superordinate terms. Even in a test of syntactic development, such as that of Berko, the use of NS is questionable. Here, too, an unfamiliar word could just as easily be used.

Earlier, in the discussion of the first experiment, it

was mentioned that some children had difficulty in understanding the pairing of a NS with a lexical item. If a child finds difficulty in pairing a nonsense phrase or word with a real entity, the central purpose of the test may be undermined. It seems reasonable to assume that this mismatch may so confuse a child that the rest of the test may become meaningless for him. Above all, the experimenter should make it clear to the child that although nonsense words or phrases are being used, the test itself is not a silly game or a waste of time. It may help to explain to the child that the experimenter is 'trying to find out some things about language' or 'working out how children learn to speak'. It is not clear in many cases whether children are made aware, even in very simple terms, of the purpose of the test they are undergoing.

The second issue to be discussed in this chapter is the use of tests of semantic development which involve children's judgements of the semantic acceptability of test sentences. NS are not involved here, but rather sentences which are semantically anomalous. Considerable research has been focussed on children's judgements of such sentences, and most of this research is based on the work of two investigators, James and Miller (1973). Whether or not subsequent researchers modify the methods used by the originators of a particular line of research, the original methods tend to persist. It appears that, in the particular research area under discussion in the remainder of this

chapter, the methods of the research originators have at times been adopted uncritically.

James and Miller (1973) presented children with a number of test sentences, asking the subjects whether, as Carr (1979:228) describes it, each sentence sounded

" ..... funny/o.k., acceptable/unacceptable". It is interesting to note that Carr modified the James and Miller technique, stating that children may not understand this type of question. Carr tested children aged between 2;0 and 3;6. She quite rightly avoided words like "acceptable", "unacceptable" and even the ambiguous "funny". Instead she chose to use "<u>can</u>" questions, such as :

"Can a dog eat?" She used six types of predicate, in groups of eight instances of each predicate type. Of each group of eight instances, four had animate and four inanimate, subjects. An example of the data obtained by Carr is given below (1979:232) :

"(1) Animate predicate <u>Wake up</u>

-Q	Can a flower wake up?	A.	No
-Q	Can a plate wake up?	A.	No eat dinner on them
+Q	Can a dog wake up?	A.	Yes
+Q	Can a cat wake up?	A.	Yes on the bed
-Q	Can a bus wake up?	A.	<u>Yes</u>
-Q	Can a bucket wake up?	A.	No have water
+Q	Can a bird wake up?	A.	<u>No, fly up</u>
+Q	Can a rabbit wake up?	A.	Yes"

Incorrect answers have been underlined. It is interesting to note that questions were framed in sequences of two with negative correct answers (-Q) followed by two with positive correct answers (+Q). In both cases where the

child has made an error, it is seen that the error occurs in a question where there is a change in the (correct) response. The child answers Yes incorrectly immediately after two questions where Yes was the correct answer. The child also answers No incorrectly after two questions where No would have been the correct response.

It is possible that the child falls, at least partly, into a pattern of responses when confronted by questions with "yes/no" answers. Also, the correctness or incorrectness of a response should not be seen as absolute, but rather as relative to the experience of the child. Thus, a child who responds yes to the possibility of a cat or dog waking up but No, fly up to the question Can a bird wake up? may simply be reflecting what his experience has been. How many adults, for instance, have seen a bird wake up? The child has, however, probably seen a bird flying upwards, so given his presumed experience, the reply is understandable.

It is instructive to consider some of the questions involving predicates other than Wake up, and to notice the cases where the judgement of the child becomes more controversial :

" -Q. Can fish be tied in a parcel? A. Yes
-Q. Can scissors bite? A. Yes
-Q. Can a dog be scribbled on? A. Yes "

(1979:232-3).

In the above instances, the line between an anomalous and a non-anomalous sentence becomes indistinct. Is it true that

a dog cannot be scribbled on? Perhaps the child has actually tried to make a mark on a dog with a piece of chalk, and has succeeded! It seems that the interpretation of sentences as anomalous or not is often a very open one. Interviewers and designers of tests should be careful in their selection of items, so that this type of marginal case does not become misleading, resulting in faulty conclusions.

Another research study will now be discussed.

Emerson (1979) tested children's intuitions regarding logical and illogical sentences in a study of children's comprehension of the word <u>because</u>. In the case of the youngest group of children being tested (five year olds), Emerson used pictures of a lady and a clown to elicit judgements of the logicality of statements :

"This is the teacher and this is the clown. The clown is a silly clown. He always says things that do not sound right or things that cannot really happen. The teacher always says things that sound sensible, things that can really happen. The clown says things like : <u>The dog drove the car</u>. That's silly, isn't it? Can you tell me what's silly about it? Can you change the sentence to make it a sensible sentence? (If no response : We could make it sensible by saying <u>The man drove the car</u>. That sounds all right, doesn't it? Or <u>The dog chased the car</u>. That would be sensible, wouldn't it?)" (1979:291).

What Emerson is doing here is actually making the judgements <u>for</u> the children, and then asking them to agree

with her. By using tag questions like isn't it? and doesn't it?, she is giving the children very strong prompts, to say the least. Also, the choice of sentence is not the best : for some children, the possibility of a dog driving a car might not be remote. Children are exposed to cartoons in which animals talk, wear clothes and even, in some cases, operate machinery. Hence this sentence may not appear "silly" to all children. The fact that, in Emerson's study, some children did not respond could indicate a real confusion on their part. There is also a problem involved in the use of the word sensible, as it is applied to the idead of a dog chasing a car. While the sentence The dog chased the car is non-anomalous, the action of a dog in chasing a car is not a sensible action. The dog could be maimed or even killed by the car, or by another vehicle.

In the previous chapter, there was a discussion of the use of toys and models as props in tests of semantic development. Children, who play with toys and models, experience fantasy often as part of their everyday routine. Cartoons, fairy-tales and other children's stories also introduce elements of fantasy into the world-view of children. Given that children have a perspective on the world which, in general, differs from an adult one, experimenters should be sensitive to this when designing tests of semantic anomaly. Children may not find it anomalous that a dog should drive a car, but may find it odd if a dog were given, for example, cat-like attributes, such

as the ability to climb trees or to spit and hiss. Researchers into semantic development would do well to bear the question of child vs. adult world-view in mind when designing test items.

Emerson does not allow for the fact that clowns do say some sensible things and teachers do say some silly things. In this respect, the method she has used, (borrowed from James and Miller, (1973)), creates an artificial context for the judgments of the acceptability of sentences. Her example of a "silly" statement by the teacher is "The cat flew away" (1979:291). Some caution must be exercised with this example : strictly speaking, it is anomalous, but it is a context-free statement. It does not, for example, sound anomalous in the following situation. An adult has just seen a dog jumping over a fence and chasing a cat. He reports this incident by saying : "The dog jumped over the fence, ran towards the cat and the cat flew away". In fact the word flew is often used metaphorically, e.g. time flew or he flew into a rage. If adults use words in this way, it is quite possible that some children do as well.

It is interesting to note that Emerson has adopted the method used by James and Miller uncritically, whereas Carr (1979) judges this method to be inappropriate. This is an important instance of two researchers in the area of child language using very different methods to conduct similar types of research. There appears to be a need to evaluate these different methods so that future studies can benefit

from their refinement.

The technique of James & Miller (1973) appears to have certain drawbacks. It is inappropriate for use with very young children who are unlikely to understand the meaning of the term acceptable. Furthermore, the description o.k. as an alternative to acceptable is too vague: to many children o.k. means 'fit', 'healthy' or 'recovered' (from an illness, for example). Similarly, funny has the connotation of 'humorous' : a sentence may, however, be semantically anomalous without being humorous, e.g. A dozen of something means eighteen. Another criticism of this technique is that it does not allow for a range of semantic acceptability : the trichotomy of

funny/o.k., acceptable/unacceptable

is unrealistically limited. A study by Howe and Hillman (1973) published in the same year as that of James and Miller allows for a broader range of opinions in respect of semantic acceptability. The Howe and Hillman article will be discussed later in this chapter. Before this, the work of other researchers who adopted the James and Miller technique will be examined.

Johnson and Chapman (1980) adopted the latter technique. Child subjects were shown pictures of a "silly" lady and an "okay" lady. The "silly" lady says things "that are all mixed up" whereas the "okay" lady says things "that are just fine" (1980:248). The subjects were then given a number of stimulus sentences and asked to judge

these as having been said by the "silly" lady or by the "okay" lady. The authors do not provide examples of the stimulus sentences used. They do, however, mention that responses are scored as correct if they matched the probable ("okay") or improbable ("silly") designation given to the stimulus sentence by the experimenter (1980:249). The adult conception of improbable is not quite the same as the child's conception of "all mixed up". The latter is associated with a far more extreme judgment than just "improbable".

It is a flaw in the research method that children are constrained to make 'either/or' judgments of sentences with varying degrees of semantic anomaly. There is no room for the child to express reservations or to indicate gradations or qualifications in the opinion he is asked to make. Since the child is faced with a sequence of choices which is often lengthy, he may resort to giving hasty or ill-reasoned responses simply to satisfy the experimenter and to bring the interview to an end.

Judgments of syntactic acceptability are far more clear-cut than those of semantic acceptability, yet it is the case that researchers have used very similar and often identical methods to elicit both types of judgment from children, as shown by research done by de Villiers and de Villiers (1972).

In this type of test of language development, the form of the question put to children is critical. It certainly

needs to be neutral, but it also should allow children to qualify their judgments. This is, after all, what competent adult speakers of a language do, and indeed are given the opportunity to do. Since pre-school children are suggestible when it comes to mere questions of eyewitness testimony (Dale et al; 1978), it follows that they might be even more suggestible in judgments of semantic acceptability. Researchers who put words in the mouths of child subjects are thus likely to find that children agree with their (the reseachers') suggestions. This likelihood, coupled with the fact that an adult researcher represents a strong authority figure in the eyes of a young child, could lead to children suppressing their *own* opinions in tests of semantic anomaly. It would appear to be important for researchers to spend more time in relaxed, *natural* conversation with children. In this way, even if it is a time-consuming process, there should be several opportunities to observe what children find semantically anomalous and how children approach problems of such anomaly.

Some children evince a keen sense of semantic anomaly, even at a relatively young age. However, this awareness is often made known in naturalistic interaction between adult and child. Horgan (1981:223) recounts episodes of a very young girl (K) correcting what she finds anomalous in the speech of her mother (M) :

"M : You're a good cook.
K : Sorry. I'm a good cookER!

and

M : I need to cut your hair.
K : HairS! Sorry!"

When a number of researchers in a given field adopt a particular method in their research, the method tends to gain acceptability. It is thus developed and extended, sometimes without being subjected to sufficient critical comment. Paul (1985) used the method of James and Miller (1973) (which Johnson and Chapman (1980) also adopted), without even a reference to these authors. In a pretest to a study of pragmatic comprehension, Paul attempted to establish that the subjects are able to "make consistently correct judgments about the semantic acceptability" (1985:170) of a number of sentences. In doing so, her subjects were shown "pictures of two ladies, a 'silly lady' and an 'o.k. lady'. Subjects were told that the 'o.k. lady' said 'o.k. things' and were given examples (A boy eats an apple, A girl rides a bike). They were told that the 'silly lady' said 'silly things' and given examples of what she might say"(1985:169).

It would appear that Paul has either omitted to acknowledge her source or has merely assumed that readers know the method being used and accept it as being correct. Thus, a method which is not uncontroversial is given a tacit stamp of approval. One wonders in this instance what a later researcher working in this field would be likely to do. It is not far-fetched to make the assumption that the

method in question will grow to be used automatically in subsequent studies. Some suggestions regarding method for future research will be given at the end of this chapter.

A noteworthy article is that by Howe and Hillman (1973) which appeared in the JVLVB. In a far more open-ended approach, the authors asked children to describe a number of sentences which contained a "violation of semantic restrictions on the verb" (1973:134). This wording was, however, not used in addressing the children. The authors then took the children's responses and used these as descriptions in the remainder of the test. It is interesting to note that children respond in various ways to semantically anomalous sentences, as Howe and Hillman report.

Some of the typical descriptions the children used were 'bad', 'silly', 'wrong', 'stupid', 'make believe' and 'doesn't make sense'. This range of descriptions is reasonably broad; certainly much broader than the silly/o.k. dichotomy. It makes allowance for there being a spectrum of semantic acceptability, with gradations from the marginally odd to the quite unacceptable. This spectrum is used for adult judgments of semantic acceptability : it seems correct to allow for it in the case of children as well.

As a final comment on the use of the James and Miller technique, it should be borne in mind that these authors worked with children of two different age groups. In the younger age group (average age approximately five years),

there was clearly less evidence of "awareness of selection restriction rules" (1973:69) than in the seven-year-old age group. It would seem to be important for researchers to bear in mind the age groups with which James and Miller worked when a decision is made to apply their technique. It may be that the technique works more effectively (has greater validity) in the case of older children. It is important that, in general, a technique used for one particular age group of children should not be applied uncritically to all age groups.

In this chapter the issue of the use of nonsense words or syllables (NS) in tests of semantic development as well as the issue of children's judgments of the semantic acceptability of test sentences have been discussed. The following are the main conclusions reached in respect of these issues :

1. Care needs to be taken that the use of NS does not confuse the child : a NS which sounds similar to a meaningful word may be given the same meaning or, at least, a similar one by the child.

2. NS were first used in a test of syntactic development. They are of dubious validity in tests of semantic development. Researchers need to investigate whether the use of NS is indeed in keeping with the aim of studying *normal* semantic development. It would appear that relatively unfamiliar *real* words should replace NS in such tests.

   A research technique which appears to be useful in one area of LD may not be equally useful in another. For example, the use of NS and ungrammatical sentences may appear useful in tests of syntactic development. However, similar methods do not necessarily "transplant" into tests of semantic development.

3. The use of NS in tests should be placed in perspective

for the child before the test begins : the child should be told the purpose of the test and the way in which the use of NS furthers this purpose. If the NS appear to do little more than confuse the child, then their use should be discontinued.

4. If many children offer similar explanations (which may differ from the ideas of the experimenter) for a certain choice of interpretation, then the experimenter should check his ideas before proceeding. What appears anomalous to an adult may not be so for a child. Moreover, adults may unwittingly oversimplify a description to the extent where it becomes inaccurate, e.g. calling a robin a "red bird" where children perceive that it is not *wholly* red.

   In tests of semantic development, it is vital to take account of the likely world-view of the *child*. Both the child's language and his perspective on reality are developing and becoming more like those of the adult. Children and adults thus hold different perceptions of semantic anomaly.

5. In adopting a research technique for assessing children's awareness of semantic acceptability, experimenters should be aware of the possible differences in age between the children in their test and the children in the prototype test.

6. In general, any research technique needs to be examined very carefully; but especially in tests of semantic anomaly care should be taken not to allow only 'polar' options. A range of semantic acceptability should be taken into account when testing both adults and children.

7. The artificiality of NS and of certain test items may vitiate the conclusions of the test. Conclusions reached in these test conditions do not necessarily hold for naturally-occurring speech and therefore for semantic development as a whole.

In conclusion, it is necessary to make some general remarks on the methods used in tests of semantic acceptability with children. Quite often, adults *themselves* would disagree about the acceptability of certain test sentences cited in this chapter. One would question why children should then be presented with these test items,

unless it were to investigate whether children displayed the same or similar types of disagreement than adults would.

Regarding the use of the James and Miller technique, it appears that a number of researchers have adopted this method without sufficient critical examination. The method of Howe and Hillman - to the extent that an experimental method in this field can be called "good" - appears superior to that of the former authors. A recommendation to all those contemplating this type of test would be to allow subjects a wide range of classificatory options - as Howe and Hillman have done.

It must again be stressed that language is a natural phenomenon and its development is best studied under naturalistic, i.e. non-experimental conditions. Semantic development in particular should be studied in context, not by using decontextualised test sentences. Naturalistic studies would appear to be the only valid means of advancing knowledge in this field.

Chapter 5

A DISCUSSION OF RESEARCH METHODS USED TO STUDY THE ACQUISITION OF THE RELATIVE CLAUSE

In this chapter, a number of research studies on the topic of the acquisition of the relative clause will be discussed. The relative clause has been selected for discussion since it appears frequently in the literature on language development. Furthermore, any discussion of relative clauses inevitably involves a mention of antecedent (nouns), which has relevance to semantics. Referents used in tests of relative clause acquisition are also often toys or models, which relates to the earlier discussion in Chapter Three of this thesis. Although primarily a syntactic feature of languages, the relative clause also has important semantic aspects. It is these aspects and, in particular, the problems associated with research methods which have been used to investigate them, that will receive attention.

Hamburger and Crain (1982) point out that a key semantic aspect of relative clauses "is an assignment of a referent to an empty noun phrase"(246). Typically, a research study of relative acquisition will attempt to test whether the child can assign the referent appropriately. For example, if an experiment is designed with two identical marbles and the child is asked to give the experimenter the marble 'which is on the chair', the child would 'pass' if he selects correctly. If, however, he were to choose the other

marble which (for example) was not on the chair but on the floor, he would fail the test.

The apparent test of syntactic mastery involved here also includes a test of the understanding of the locative 'on' and the word 'chair'. An obvious flaw of this test is that the child has a 50% chance of passing if he guesses correctly. To allow for the possibility of guessing, the experimenter can modify the test to include more marbles (in different positions) and use different instructions e.g. Give me the marble which is on the floor, and Give me the marble which is on the table. However, as will be shown later in this chapter when experimental conditions become more complicated other sources of error are often created. It will be argued in this chapter that the design of experiments to test relative acquisition is extremely problematic, and that some such 'tests' do not really focus on what they are intended to investigate.

A crucial preface to any discussion of the relative clause is the distinction between the restrictive and the non-restrictive variety. For ease of reference, these two types will be denoted by the respective abbreviations, RRC and NRRC. Examples of these would be:

1. Give me the marble which is on the chair.

2. Give me the marble, which is on the chair.

The RRC 1 would be used if there were more than one marble i.e. to remove ambiguity of reference. On the other hand, the NRRC 2 would be used if only one marble was

present in the room; the comment 'which is on the chair' is actually gratuitous. However, for an adult addressing a child, such a comment might be used to guide the child to the marble or even to reassure the child in some way. Phonetically, the distinction between 1 and 2 is marked by the pause in 2 after marble. This pause acts as further confirmation to the listener that the relative clause is in fact a non-restrictive one.

Hamburger and Crain (1982:247) refer to the early prototype relative clauses which children use. Such "protorelatives", as these authors call them, place "a restriction on the set referred to in the noun phrase". As regards syntax, the form of these "protorelatives" consists of (at least) "a verb phrase within a noun phrase" (1982:247). The examples given by these authors are interesting in that they illustrate how the child in question (a two-year old girl) uses relative-like constructions to refer to objects whose names she does not have in her lexicon:

	Utterance	Referent
1.	This my did it	a painting
2.	That's a flush a toilet	a toilet handle
3a.	Look- a my made ...	a toy construction" (1982:247)

Utterance 1, "This my did it", is called a "protorelative" because it is a paraphrase of the utterance "This is what I did". Similarly, utterances 2 and 3a are paraphrases of "That's a thing which is used to flush a

toilet" and "Look at what I made", respectively. In all three cases, the child appears to lack the ability to construct grammatical relative clauses.

All of the above utterances are generically more akin to the RRC than to the NRRC, although it is necessary to know the context of each utterance in order to interpret it as a relative clause. In this type of research (a longitudinal study of one child over a period of one year), it is possible to piece together various bits of evidence for the acquisition of relative clause constructions. Unfortunately, in the other, experimental type of study, conclusions must stand or fall on a fairly restricted set of data. It is this type of study i.e. the experimental one, to which attention will now be given.

A common experimental procedure for testing children's comprehension of relative clauses is to ask them to act out their interpretation of a sentence, e.g.
<u>The horse kicks the man that pushed the dog</u>. The relative clause is also often embedded in the main clause:
<u>The man that pushed the dog is kicked by the horse.</u> In this type of experimental test, toy models of animals are often used. The child is asked to manipulate these toys in a sequence which enacts the events described in the test sentence. This method of testing children's comprehension of sentences was given prominence by Carol Chomsky who used it in tests of syntactic development (Chomsky;1969). The rationale behind such tests is that they provide evidence

that the child actually understands the sentence (and the relative clause in particular) if he can provide a correct enactment thereof. As Hamburger and Crain (1982:251) point out, such sentences contain both an assertion (i.e. the statement in the main clause) and a presupposition (the information in the relative clause). This observation highlights the semantic aspects of the test sentences.

In their discussion of earlier research into relative clause acquisition, Hamburger and Crain (1982:255) state that five-year-old children often appear to misinterpret the restrictive relative clause. Children of this age, tested in experimental settings, do not appear to have mastered the RRC. The authors put forward the view that faulty testing methods may actually cause children to misinterpret the RRC. In support of their argument, they invoke the Conversational Maxims enunciated by Grice (1975), specifically the Maxim of Manner, which they paraphrase as follows:

> " Manner : be perspicuous
>
> a. avoid obscurity (of expression)
>
> b. avoid ambiguity
>
> c. be brief (give only necessary information)
>
> d. be orderly "
>
> (1982:255).

With respect to submaxim a., the authors examine test sentences used by the experimenters Sheldon (1974) and Tavakolian (1978), in which there is evidence of "the present tense with nonprogressive aspect" (1982:256) as in,

for example:

The lion jumps over the cow that the horse bumps.

Hamburger and Crain find that the use of this tense is obscure (in the Gricean sense) in that it is normally used in the case of "definitions and recurrent events, but is unnatural, hence somewhat obscure, in the situation of experiments" (1982:256). They argue that:

"In ordinary adult conversational descriptions of on-going non-recurrent events, the progressive is preferred ('Look! The lion is jumping'.)" (1982:256). The authors would prefer that the test sentences be in the "infinitive tense"(sic): "Perhaps the experimental setting makes it appropriate to use stage directions, but then one would expect the infinitive ('Make the lion jump')" (1982:256). The general point being made, that the choice of verb tense should be appropriate, would seem to apply to all experimental language studies of this type. It is an important point, and one which is not emphasised sufficiently in descriptions of research methods.

Another Gricean submaxim that can be violated by test sentences is ambiguity. This is a general problem in all language tests, but one which is more serious in tests of language acquisition. An examination of a number of test sentences reveals that there are many ways in which ambiguity can occur. Sheldon (1974) tested children's comprehension of the RRC by asking them to act out sentences, using toy animals. Although the sentences appear

simple and clear to an adult reader, the children in question (aged 3 to 5 years) found difficulty in comprehending them. Sheldon used four sentences, which he labelled SS, SO, OS and OO respectively. As described by Hamburger and Crain (1982:252), the S and O stand for subject and object (of verb and preposition) and these abbreviations are used as follows: the first letter of the pair stands for the position of the matrix noun phrase that bears the relative clause, whereas the second letter stands for the position of the empty noun phrase inside the relative clause. In this context, the word <u>empty</u> means non-occurring or implicit. In the SS example set out below, the noun phrase <u>the dog</u> would be "empty" since it is the implicit subject of <u>jumps</u> in the relative clause. The sentences which Sheldon used were:

SS : The dog that jumps over the pig bumps into the lion.
SO : The lion that the horse bumps into jumps over the cat.
OS : The pig bumps into the horse that jumps over the cat.
OO : The dog stands on the horse that the giraffe jumps over.

Sheldon used these sentences to test whether children find it more difficult to process sentences of the SS and SO variety, in which the main clause is interrupted by a subordinate (relative) clause. As Hamburger and Crain (1982) mention, this "Interruption Hypothesis" was first proposed by Slobin (1966).

Sheldon's tests actually disconfirmed this hypothesis,

since he found that children had greater difficulty in interpreting the OS and SO sentences. Sheldon explained this by making a "Parallel - Function Hypothesis". According to this idea, "sentences in which the same noun phrase is used with different, 'non-parallel', grammatical functions in the two clauses (main and relative) will be more difficult to comprehend than sentences in which a noun phrase plays the same, 'parallel', function in the two clauses" (1982:252).

Whichever of these two hypotheses is a better explanation from the syntactic point of view of children's misinterpretations of sentences involving the relative clause, the problem of ambiguity is definitely a semantic factor in such misinterpretations. Each of the test sentences contains *three* nouns (and hence three possible actors) whereas each sentence contains only *two* verbs. This is a potential source of confusion for the children, even more so because of the unlikely situations which are portrayed in the test sentences. Consider again the sentence:

The dog that jumps over the pig bumps into the lion.

Each of the animals mentioned is capable of bumping, and each is capable of jumping over the others (although a pig would find this harder than the other two). Thus, a child faced with interpreting this sentence has little in the way of semantic clues at his disposal. Compare the above sentence with the following:

The car that runs over the dog crashes into the wall.

Syntactically, there is nothing to distinguish these sentences. However, their semantic 'structure' is very different. Cars run over dogs (but not vice versa, except, possibly in scrapyards) and cars also crash into walls more frequently than dogs do. It would seem, therefore, that the latter sentence would present far fewer problems of interpretation for the child. Apart from anything else, it describes a sequence of events which has a far greater probability of occurrence than that described by the former sentence. Since children's language is developing slowly towards adult competence, it appears to be fitting that language tests should give preference to test items that are realistic. It is after all primarily the child's language, not his imagination or creative ability, that is being studied.

As is often the case in studies of semantic development, researchers are quick to propose alternative theories to account for the apparent difficulty children experience with different test items. The researcher Tavakolian (1978) took another stance to oppose Sheldon's Parallel-Function Hypothesis, on the basis of data revealed by her own experiments. In research described by Hamburger and Crain (1982), Tavakolian explained an error type which occurred frequently in her data as arising from "the children's use of a set of rules needed anyway to explain (their) correct interpretation of missing noun phrases in

conjoined clauses"(253). Using this "Conjoined-Clause Analysis", she explained that, for children, the distinction between that and and would be lost in such sentences as:

(1) The sheep knocks down the rabbit and stands on the lion.
(2) The sheep knocks down the rabbit that stands on the lion.

Since children have, in any event, to infer the sheep as the subject of the second clause in (1), it would be a logical assumption, according to Tavakolian, that a similar inference could result in an erroneous interpretation of (2).

Another researcher, Legum (1975), has what Hamburger and Crain think is an "intriguing explanation" (1982:254) for this type of error: the idea which Legum proposes is that once "a child gets ahold of an animal, he tends to hang onto it. By this reasoning, known as the 'Bird-in-Hand Strategy', there would be a tendency to use the same subject in each clause". (1982:254). Legum's explanation may or may not have validity, but it does expose a potential flaw in the thread of method which runs through the series of experiments. If the acting out of their sentence interpretations is the *only* way the children are using, or being permitted to use, then the experimental method is open to serious question. It is this type of flaw in research method that often passes unnoticed by a series of investigators, resulting in a large volume of redundant research.

Subsequent research indicates that quite different

responses to OS relatives like the one that gave rise to Tavakolian's Conjoined Clause Analysis can be achieved by altering the test sentences so that the probability of an erroneous interpretation is reduced. Instead of using the sentence

(2) The sheep knocks down the rabbit that stands on the lion,

researchers used the sentence

(3) The girl pushed the cow that bit the horse.

A reduction in the relative number of errors of interpretation with (3), compared to the number of errors associated with the interpretation of (2), is reported by Solan and Roeper (1978). Apart from the change in the tense of the verbs from present to past, the fact that girls are not likely (in theory) to bite horses, makes for the lowered probability of girl being inferred as the subject of the relative clause in (3). Indeed, to an adult, the probability of either a girl biting a horse or a sheep standing on a lion would be virtually zero. However one views it, one is still faced, in sentence (3), with a description of a highly unrealistic situation.

It is the submission of this thesis that the contrived nature of tests of semantic development results in test items being selected which, because of their arbitrary properties, lack the clarity which is associated with sentences used in natural speech. Not only are these sentences removed from a natural speech context, they are

also often confusing because they have been chosen without regard for their ambiguity. They also tend to represent distortions of reality. In everyday speech, many sentences are disambiguated by their context. This is not the case in tests of semantic development such as the one for relative clause acquisition described in this chapter. A child who is asked to interpret an utterance in a sequence of test items is in a comparable position to an adult being asked to decode a sequence of words which, if not actually generated at random, show little syntactic or semantic cohesion. This analogy may sound extreme at first, however, normal comprehension occurs in a real-world context. Therefore, comprehension of *sentences in isolation* is not necessarily a valid measure of semantic competence.

Another researcher, Goodluck (1978), discovered that by reducing the number of animals in a test item from three to two, the comprehension of relative clauses was facilitated. This was achieved by either substituting a lexical item denoting an inanimate object for a lexical item denoting an animate one, or by making use of an intransitive verb. Sentences cited by Hamburger and Crain (1982:254) illustrate this approach.

"The dog kicks the horse that jumps up and down.
The pig bumps into the sheep that jumps over the fence".

It would thus appear that, even with sentences generated in an artificial situation, the choice of lexical items has a bearing on the ease with which such sentences are

understood. Goodluck and Tavakolian were led, from these considerations, to propose "a processing - rather than a competence - account of children's errors in relative clause production", (Hamburger and Crain, op. cit.:254). It would seem that the point being made here is valid. Some sentences appear to be difficult to interpret because of the probability that children will incorrectly identify the subject of the relative clause. By reducing the number of nouns denoting animate objects, it may be possible to achieve a less obscure sentence.

Hamburger and Crain (1982) make one of their most telling indictments of the "acting-out" type tests of children's relative clause acquisition when they describe the violation of the Gricean submaxim of brevity which occurs in all of the tests described up to this point. In the experiments with toy animals, children were provided with only *one* animal of each type. The use of a RRC is thus inappropriate to the context. For, as Hamburger and Crain (1982:257) point out : "with only one horse available, the assertions of 17a and 17b are the same, so the relative clause in 17a provides unnecessary information", counter to the Gricean admonition to be brief :

"17a The cow bumped the horse that tickled the cat
17b The cow bumped the horse" (1982:257).

This again is clearly an instance where the sentence used in a test is inappropriate to the context. If only one

toy horse is available to the child, then the RRC that tickled the cat is redundant.

However, there is potentially a greater problem than redundancy here. As Hamburger and Crain (1982:257) correctly point out, the (child) listener receives an instruction to restrict the set of potential referents (horses), of which there is only one, but is unable to execute this instruction. This may set up considerable confusion in the listener's mind and, in fact, render him unable to act out his interpretation of the sentence.

The obvious change required in the experimental method is to have more animals (horses) so that the child is faced with circumstances "in which the relative clause is indeed needed to communicate what is to be done" (Hamburger and Crain; 1982:257). That one has to adapt one's method, is once again due to the artificial nature of the experimental situation. In the course of natural speech situations, the language used is chosen to fit the context, and not vice versa. This example underlines the need for experimenters to exercise caution in test construction. One suggestion would be to observe a group of older children in a variety of communicative contexts, and to record instances where the RRC was used appropriately by these children. This could, for example, involve a child asking another to give him object X which has the distinctive characteristic Y.

A similar contextual arrangement could then be made for groups of younger children, and the use of RRC could be

noted for these groups. Preferably, experimenters would merely observe and record the corpus of child speech. However, if insufficient instances of RRC usage were to be found, it could then be decided to attempt to elicit RRC's from the children by using suitable questions or prompts. Hamburger and Crain (1982:271) mention that children are able to use adjectives or prepositional phrases to avoid having to use a RRC. They may say, for instance "the blue W; the W with the hat" instead of (my examples) the W that is blue or the W that has a hat.

To overcome this particular problem, the authors suggest using referents who can only be distinguished by means of an act which one (but not the other) did. The child is then constrained to use a RRC to refer correctly to one or the other referent. They illustrate this by means of a mini-story followed by a question. It is also interesting to note the use of the past tense in this type of elicitation test. (Earlier in this chapter, reference was made to the past as a more appropriate tense than the present with nonprogressive aspect for the phrasing of test items on relative clause acquisition). Here is their example:

" 24. There were two brothers. One boy ate dinner and the other went to bed without dinner. Which boy was sad? (Typical response : 'the one that didn't eat dinner.')" (1982:271).

Although this example does not provide a watertight

test - a child may, for example, reason that the food was unpleasant and therefore the boy who ate dinner was sad - the authors nevertheless appear to have hit upon a useful method of eliciting a RRC. Other methods which they suggest include the use of a blindfold on a listener. The child subject has to use language (and more particularly, a RRC) to convey meaningful information to the listener. Mere gestures (e.g. pointing) do not help in these circumstances. The example given by the authors is that of two identical animals (walruses) and a zebra. The experimenter uses the zebra to "tickle" one of the walruses and then instructs the child subject to ask the blindfolded listener (L), to pick up the walrus being tickled. The experimenter might say, for example:

> "Tell L to pick up this one" (1982:270).

The listener L cannot see which walrus he is being asked to pick up, and has to rely on an appropriate clue (which the authors hope will include a RRC such as :

> "Pick up the walrus that the zebra is tickling" (1982:271).

Presumably, the proximity of the agent and patient animals once the blindfold on L has been removed, together with the verbal clue in the form of the RRC, would be sufficient to enable L to carry out the instruction correctly. However, the problem of artificiality is inherent in a situation like this, and it is possible that the child subject would not make use of a RRC. One is

reminded here of Chomsky's discussion of linguistic creativity (1959:35), in which he makes the point that it is impossible to predict utterances in a given situation. This feature of language causes difficulties in any type of research which seeks to elicit utterances from a subject.

The authors claim "success rates" of RRC elicitation of 73% and 72% respectively, using the methods of mini-stories and blindfolds (1982:271). However, the methods used remain artificial and it is difficult to gauge whether the linguistic performance shown by children in such experiments is a reliable measure of their competence in RRC production. All that can be said with any confidence is that, under certain fairly restrictive conditions, children of a certain age display a certain percentage of RRC production. The authors admit that children as young as "under 3" have been observed to produce relative (or relative-type) clauses spontaneously (1982:272). Nevertheless they maintain that elicitation procedures are valuable :

"standard elicitation conditions, like those here, have two potential advantages over reliance on spontaneous production : readily replicable results and greater confidence and precision with respect to the meaning a child attaches to a relative clause" (1982:272).

It should, however, be emphasised that there is a distinct disadvantage to using a technique of relative clause elicitation : one's conclusions are confined to situations which are contrived, and the "meaning" which the

child "attaches to the relative clause" is thus similarly restricted. In their quest for aggregate results and "success" percentages, the authors appear to have laid insufficient stress on the natural (often piecemeal) acquisition of relative clauses which appears to be the norm for children.

At this point it is instructive to reflect on Hamburger and Crain's opening sentence (1982:245) in their article on relative acquisition :

"A child can partially acquire a concept and/or the means to express it. One might then say that relative acquisition has taken place : relatively more than nothing, relatively less than everything". This facetious comment has relevance to the question of the study of child language: individual progress in acquisition is difficult to study and more difficult to measure. Since individual differences are seemingly considerable, discussion of group percentages and "success" rates should be tempered by this knowledge. Instead of attempting to arrive at a law of 'mass behaviour' in such a delicate area of language study, it may be preferable to concentrate on individuals, in naturalistic settings, and to compare one's observations with those of other students of language acquisition. However, having made these points, one needs to acknowledge that, if research in experimental settings is to continue, then such research can only benefit from a refinement of its methods. Given that such research will in fact continue, one can at

least attempt to isolate those methods having as few obvious defects as possible.

The study of relative clause acquisition reveals that many problems beset researchers in this area. It is vital that the distinction between the NRRC and the RRC be clearly understood by researchers. The design of tests is difficult since both the context and the test sentences, being contrived, are often inappropriate or, at worst, ambiguous. It is important that consideration should be given to the Gricean Maxim of Manner, so that sentences are not only unambiguous but are also free from obscurity of expression. The submaxims of brevity and orderliness are also worthy of note. In particular, it would seem that the tense used in the construction of test sentences should be appropriate. Research methods need to be carefully scrutinised to ensure that a weak or faulty method is not repeated. It appears that researchers tend to become locked into debates over why subjects do better with one type of RRC than they do with another, without realising the importance of the contexts in which the different clauses are used.

In conclusion, the following further points should be noted :

1. Tests of relative clause acquisition should be based on realistic situations.
2. Naturalistic observation should be explored more thoroughly as a means of studying relative clause acquisition.
3. Elicitation of relative clauses is a particularly difficult method of research : its validity is also

very restricted.

4. Researchers should not become preoccupied with percentage results, especially in this area of language study. Individual differences are also important, and not just laws of mass behaviour, in a given situation.

Chapter 6

AN ANALYSIS OF A NUMBER OF NATURALISTIC STUDIES OF LANGUAGE DEVELOPMENT

In previous chapters, there has been a critical discussion of the use of tests to study language development. The purpose of this chapter is to examine other methods of studying language development and semantic development in particular.

Naturalistic observation of language development has a relatively long history, going back at least as far as the previous century to Sully's (1896) anecdotal accounts of child speech. A number of prominent students of language have made detailed analyses of the speech of their own children, cf. for instance Burling (1959), Leopold (1949), Weir (1962), Smith (1973) and Halliday (1975). Studying the speech of one's own children has the advantages of naturalness and both intensive and extensive observation opportunities. Points of disadvantage include the possible bias of an interested observer and the lack of replicability of many of these studies. The process of data-collection in such a study is often a painstaking one. The latter feature of naturalistic studies has no doubt inclined researchers (especially those with publishing deadlines) towards the experimental methods discussed in earlier chapters. From a practical point of view, it is therefore easy to realise why experimental studies of language development

began to proliferate. Other important influences were the growth of cognitive psychology as a subject and the popularity of the use of statistical methods as part of general research techniques in the second half of this century. It is a real difficulty to attempt research over a long period on the language development of a child; if the child is not one's own, the problem is greater still. The question of having sufficient time for such a study is but one of the considerations confronting someone wishing to do this type of research. An equally frustrating difficulty is that children's speech is often fragmentary, with long periods of silence in between. If one's research and observations are to be natural, then one cannot 'set up' situations in order to elicit a corpus of data; one has to depend on a naturally-occurring corpus which arises out of the child's normal routine. The gathering of data in such circumstances is a cumbersome procedure, requiring an often tedious sifting of what is relevant from what is not. The records involved can understandably become unwieldy and can tend to overwhelm the researcher.

However, the sheer bulk of the task is not in itself a reason for turning aside : the researcher needs to weigh the intrinsic merit of his approach against the effort required to accomplish the work. It should also be realised that, in the past, naturalistic observation was often, if not exclusively, carried out by observers who did not share specific research goals. If a more co-ordinated approach to

this type of research were to be used, the data and conclusions of one researcher could be more readily compared with those of another. In short, greater co-operation between researchers in this mode of research would be likely to lead to improved and more frequent insights. Another advantage of co-operation in this type of research is that it should lead to less unnecessary duplication of the type that occurs when students work separately. Time could thus be used more efficiently and, more importantly, researchers would experience mutual support. For example, a number of students with the single research focus of the overextension of the lexical item cup could share their observations of the use of this item by two-year-olds in everyday situations. It appears likely that a reasonable degree of consensus as to the use of this item would be reached in a relatively short period. Attention could then be given to another research focus by this group of people.

At this point it is illuminating to discuss the methods used by a number of investigators using the naturalistic mode. Important similarities as well as differences in their respective methods will also be stressed. The area of emphasis will be the process which is used, rather than the product of an individual piece of research. In the concluding chapter, an attempt will be made to highlight those approaches which appear to be more effective from a research viewpoint, and also to suggest modifications in research procedures where appropriate.

One of the more recent and better documented naturalistic studies of semantic development was performed by Halliday (1975). In this research, Halliday studied the semantic development of his son, Nigel, over a period of nine months (from the age of nine months to that of eighteen months). This is a period of development which is difficult to study for a number of reasons : the child is also beginning to walk (at about age one) and has a short attention-span, besides being not fully articulate. Much of the time of the child is spent with the mother, so that an outside observer would have to obtain the mother's permission to observe her child. Fortunately for Halliday, this was not a problem. However, these factors need to be borne in mind, when this type of study is being planned.

Halliday's study concentrated on semantic development. In itself, this approach is not remarkable. What is remarkable, however, is Halliday's distinction between an "adult-oriented" and a "child-oriented" perspective on the way in which the child enters into "structural combinations" (1975:2), whereby he puts together linguistic "elements". This feature of Halliday's work is also noteworthy in that it makes allowance for the part played by <u>the child</u> in learning a language. The child is an active and purposeful participant, in Halliday's view, in his own language development. The stress laid on the child's role has a natural analogue in the method used to study language development. Instead of setting up short synthetic testing

sessions to study some feature(s) of the child's development towards one or other adult language norm, the approach taken by Halliday was to observe and record the child's language in a variety of everyday situations, over a period of time. These observations were then interpreted in terms of their relevance to the child and to the development of the child's linguistic system. The child is thus studied as a child, in his own right, and in his own world, and not as a minature adult.

Halliday also stresses that his approach to the study of child language development is a "functional-interactional" one (1975:5). He views the child's developing language as a "progressive mastery of a functional potential" (1975:5) in which the child comes to realise and use particular linguistic structures to achieve communicative objectives. Halliday relates the functional to the interactional approach via a sociological explanation. In order to achieve certain aims, the child uses language to (inter alia) "regulate the behaviour of others" (1975:5).

However, the approach which Halliday takes is basically a semantic one. He views the learning of language as the learning of a semantic system, but does not restrict this system to meanings of words only, but to a system which "is to be seen as a semantic potential. It is a range of possible meanings, together with the means whereby these meanings are realised, or expressed" (1975:8). More

specifically, he states that his notion of a functional approach to language learning "refers to the general notion that the child learns language as a system of meanings in functional contexts" (1975:9). Context is therefore central to Halliday's notion of semantic development - he is meticulous in relating the vocalisations of his son to the context of utterance. Naturalistic observations of his son's utterances in context constitute Halliday's research data.

Halliday's essentially child-centred approach differs from that of many researchers working in experimental modes. Because of the focus on what the child is trying to achieve, the descriptions which Halliday uses are different from those common in experimental approaches. In a sense, Halliday rejects adult-centred labels for the child's language : "the child already has a linguistic system before he has any words or structures at all. He is capable of expressing a considerable range of meanings, meanings which at first seem difficult to pin down, because they do not translate easily into adult language, but which become transparent when interpreted functionally, in the light of the question 'What has the child learnt to do by means of language?'" (1975:6). Halliday interprets early semantic development as the growing ability to express a *range of meanings* rather than as the ability to utter words which an adult could recognise.

Halliday allows for the development of child language

into adult language by postulating a three-phase system, through which the child's initial "functional-linguistic system" grows towards adult language, or competence, in linguistic terminology.

In Phase I, he studies his son, Nigel, during the period from nine to eighteen months, which period he sub-divides into six stages of one and a half months each. Each such stage has a label, e.g. NL 0 stands for Nigel's language at 9 months, whereas NL 6 stands for Nigel's language at 18 months (1975:12). Of particular interest to this thesis is the way in which Halliday sets out his method of data-gathering. Each stage of language is described, and Halliday stresses that, since Nigel's language "lacks both structure and vocabulary during these stages the word 'description' rather than 'grammar' is used for each stage" (1975:11).

Halliday describes his method as follows : "I made notes of the child's utterances, using only the traditional equipment of the field worker, well suited to this stage, of notebook and pencil. I listened in, sometimes taking part in the situation and sometimes staying outside it, hiding behind doors and furniture; and I noted down any meaningful expression that I thought I was observing for the first time. Then I also noted down expressions which I considered to be the same as those I had observed before, not every time I heard them, of course, which would be impossible, but at fairly frequent intervals, the point being that at this

stage it is not enough to assume that because some item has been observed to occur it is is now part of the child's linguistic system" (1975:11). By noting down the use of an expression in its context of utterance, Halliday is able to obtain a comprehensive perspective on his son's semantic development. Random vocalisations are treated as such, and only expressions which are consistently used in similar contexts are taken as evidence of semantic development.

It is interesting to note that, unlike many experimenters, Halliday makes no use of a tape recorder in his research. His method is simple - merely pencil and paper - and he manages to observe the child unobtrusively at times. His phrase : "I listened in ..." speaks volumes for his method. Halliday is also selective in what he records, thereby avoiding an unmanageable volume of detail, which could present a problem of analysis. He does, however, make a note of the first instance of every expression which he regards as meaningful. Regrettably, the criteria for an expression to be "meaningful", are not spelt out here by Halliday. One is also made aware of the subjective nature of this type of method : what Halliday regards as being "meaningful" may not be so for the child uttering the expression. However, the balancing factor in Halliday's favour is that his method involves the child in everyday, natural situations. Furthermore, Halliday is bent on systematising the child's utterances, and therfore relates each occurrence of an utterance to previous instances. In

this way, he is able to piece together a composite description of the child's semantic development.

It is instructive to take a closer look at Halliday's method : he chose a period of six weeks as the "optimum interval" (1975:12) for separating the descriptions of his son's developing language. His reasons for choosing this period are twofold : choice of a longer period would have resulted in "certain significant steps in the development" (1975:12) being omitted; whereas choice of a shorter period would have resulted in the omission of items simply because they had not occurred in the short period of time chosen. Halliday's choice of six weeks is of course arbitrary and his justification for this period is based on subjective judgments of its appropriateness. If a similar study were to be carried out, thought would have to be given to a possible revision of the length of this period. Furthermore, since semantic development is not necessarily linear growth over time at the various ages of the child being studied, it may be found to be suitable to have stages of different durations in such a study.

Early in his book, Halliday continually mentions that the early language, or protolanguage, which he observed in his son differs markedly from adult language. So much so, that Halliday coins a number of expressions to describe the child's system. The main difference between the systems of child and adult is described as follows:

"the system differs from the adult language system in

that it has no intermediate level; it has no stratum of grammar (we should say lexicogrammar, since this stratum includes vocabulary) intermediate between the meanings and the sounds" (1975:12-13). It is the ability to link sounds systematically with meanings that Halliday views as the nub of semantic development at this stage of the child's growth.

Halliday also distinguishes between <u>child</u> and <u>adult</u> in terms of language <u>content</u> and language <u>expression</u>, as follows:

"the particular expressions are not, or at least may not be, imitations of the adult language .... It is a system of vocal postures ... which are taken up by the articulatory organs, general configurations rather than the specific bundles of contrastive phonetic features which make up the elements of the adult sound system.

By analogy, we can make the same point about the content ... We cannot match the child's meanings with the elements of the adult semantic system, which are again too specific. What is needed, in similar fashion, is a kind of postural notation for the content ... The content, in other words, has to be specified in relation to the functions of language"(1975:13).

It is not the central concern of this thesis to dwell on theoretical considerations (and on considerations of phonological development in particular). However, the points made above by Halliday do bear consideration, for they have consequences for the methods which subsequent

researchers into child language may choose. If one attaches importance to Halliday's point about the content of the child's linguistic system, then analytical methods which employ semantic features used in descriptions of adult language may not be entirely suitable for the "language" of children at the stages described by Halliday. On the other hand, since children commence their transition into a more adult-like language at different ages, the "conventional" descriptions can never be too far in the background. Halliday is more concerned with the developmental process than on the product, and, since his research focusses sharply on the language development of one individual, it is understandable that a more fine-grained description results from using the less conventional developmental labels.

One of the insights into semantic development which Halliday achieves in his study of his son is recognising that, for an item to be part of a child's linguistic system, "there should be a constant relation between the content and the expression"(1975:14). Halliday refers to this requirement as "systematicity". He gives as an example the item "nananana" which his son uses if and only if he expresses the meaning "I want that thing now" (1975:14). It is one of the advantages of using a naturalistic method of research that regularities in speech such as this one, are observed. Even if the child does say nananana once in the course of an experimental situation, the researcher may either gloss over the occurrence or not have the opportunity

to confirm what he suspects to be the meaning of this item. Despite the fact that children do use vocalisations like this, their usage is not static, and gradually a more acceptable (in adult terms) expression will begin to replace the crude prototype. The naturalistic researcher has the advantage of being able to observe this developmental process.

Besides his requirement of systematicity, Halliday also proposes the requirement of "functionality" - this is taken to mean that "the content should be such that it can be interpreted by reference to a prior established set of functions" (1975:15). By "function" in this context, Halliday means "one or other of the things that the child is making language do for him" (1975:15).

This requirement is the cornerstone of Halliday's theory of semantic development. After reflection on the "language" of his son, he hypothesised a number of "functions" which, although they refer to the communicative context in which the child finds himself, are more general in scope and have applicability to adults as well. According to Halliday (1975:16), social man is served by language in that it enables him to interact with his fellows in a number of different ways. Halliday's various observations of his son in communicative contexts provided him with a keen insight into how the child, despite his lack of "adult" language competence, managed to use his speech to a number of general ends. By being able to observe and

reflect on the vocalisations of his child as they occurred in a natural context, Halliday was able to connect the expressions which Nigel used to the communicative needs of the situation, especially in the case of semantic development. Halliday is careful to distinguish between his son's semantic potential at a given developmental stage and his "total semiotic potential" which Halliday defines as "the information system that is embodied in the whole of the child's behaviour" (1975:23). Halliday's method of naturalistic observation is well suited to his purpose of studying his son's semantic development: he is able to relate linguistic aspects to non-linguistic behaviour (like body-language).

It is not in place to discuss the various functions of language to which Halliday refers - these are properly the domain of sociolinguistics and beyond the scope of this thesis. What is more relevant is to look carefully at Halliday's method of studying Nigel's speech.

Halliday mentions that his son, in at least one instance, used a sound which he (Nigel) made in his pre-sleep vocalisations "as having the meaning of withdrawal, and more specifically 'I'm sleepy'..."(1975:23-4). Thus, a naturally-occurring sound, made by the child himself, became a unit in the child's linguistic system. Another astute observation by Halliday is that Nigel, at about the age of 18 months, would use certain items in contexts of "observation, recall and prediction"(1975:27). Nigel would

recall a word he had used up to three weeks previously, and also use words to refer to objects which he was expecting to see (while out walking, for instance). At this age, a large number of object names appeared to enter Nigel's vocabulary. Because Halliday was able to study his son's speech over a long period, and was not constrained by a series of interviews of fixed length, he was able to make observations about Nigel's ability to use words in a situation of recall or predicting. This is a further argument for the naturalistic method of research, especially in the case of semantic development.

Halliday's method also enabled him to study the transition from Nigel's use of his own linguistic system to that of adults. Two important points emerge from this study:

(a) Halliday noticed the stage at which Nigel began to use language to inform his listener. Nigel used the interrogative form "to convey information that he knew the hearer did not possess" and the declarative form to comment on something which was already known to the hearer (1975:32).

(b) There is an account of how Nigel uses language in phase with his construction of a social system (1975:121) and how, in particular, his developing concept of time is related to his use of the names of days of the week.

Both of these points are illustrated by examples which Halliday provides of the spontaneous speech of the child. There is no need to design an artificial situation : the developing language reveals itself in situations of everyday life. More particularly, Nigel's semantic system develops in response to his needs as a social being.

Halliday places his study in the context of other studies in linguistics, and of child language in particular. He has developed the ideas of Brown on the "'rich interpretation' of children's language"(1975:3) and Fillmore on case grammar (1975:4). For his sociolinguistic theories he has borrowed from the work of Bernstein (1971,1973) and certain of his descriptive techniques are derived from Firth (1975:7). For Halliday, the social-interactional model of language development is pivotal to his theory - he views cultural acquisition as the milieu of language acquisition.

Unlike other researchers, Halliday does not make frequent comparisons between the language of his son and that of other children. This is because his chief concern is the construction of a general theory of semantic development based on his own observations of Nigel's speech. It would have been interesting had Halliday been able to apply his theory to the language of children other than Nigel. Nevertheless, his work stands out as a naturalistic study of child language and will provide future researchers with sound empirical data on which to proceed. It represents a milestone in the overall study of child language.

In a far less intensive longitudinal study of the language development of one child than that of Halliday, Burling (1959) gives an account of how his son Stephen developed bilingualism in English and Garo, an Indian language. Burling brought his family into a Garo-speaking

environment when his son was sixteen months old, and the family spent two years there. It is interesting to note that Stephen was at the one-word stage in English when he came into regular contact with Garo. As Burling (1959:45) points out, Garo words rapidly entered his son's vocabulary, eventually overtaking the English words in number. There was thus a rapid period of lexical acquisition of Garo brought about by the new environment.

Burling (1959:46) ascribed the predominance of Garo to the fact that Stephen's mother was in hospital for about two months, when Stephen was about 1;9 years of age. This environmental factor was also made more influential by his father speaking to him frequently in Garo and the fact that even after his mother's hospitalisation, her illness "forced him into greater contact with Garos than might otherwise have been the case"(1959:46). Equally, however, the removal of his son from contact with Garos at the end of the Burlings' stay in India resulted in Stephen rapidly losing his fluency in Garo with the result that, within six months of leaving India, Stephen was "even having trouble with the simplest Garo words"(1959:63).

Burling was able to keep detailed records of Stephen's speech for the first eighteen months of their two-year stay in India, concentrating his attention on phonetics and morphology, but commenting on semantics as well. He mentions a number of difficulties that are associated with the study of child language, among which are "the poverty of

the children's vocabulary" and "the difficulty of getting them to repeat forms"(1959:45). On the positive side, Burling mentions that both child and parent enjoy the close interaction that such a study involves. It is also illuminating to consider nonlinguistic effects, such as the absence of the mother, on the child's language development. Burling also refers to an interesting aspect of Stephen's language development in that during his sleep he would speak in Garo, not in English (1959:63). It is this type of observation that can only be made in a naturalistic study of language development, experimental methods invariably involve the speech of children who are awake. By failing to capture child language in all its possible modes, an experimental study can at best give only a partial account of such language.

Regarding his son's semantic development, Burling points out that this tended to lag behind both phonetic and syntactic development. Stephen used "several numeral classifiers in Garo quite readily" (1959:60) as well as "color terms in both English and Garo" (1959:61) without showing much understanding of the terms he was using. Although he was using "color terms" as early as 1;10, it was only about a year later, at 2;9, that he began to use these terms appropriately. According to Burling (1959:61) the grasp of the correct meaning of these terms occurred suddenly.

Stephen's use of spatial terms was more precise than

his use of terms denoting time, at corresponding ages. Burling (1959:61) also mentions the onset of metalinguistic awareness in his son at the age of 2;9:

"He would occasionally start to speak, pause, point to something, and ask wats dis; receiving an answer, he would proceed to use the word in his sentence." Although Stephen was aware that he was using words, some usages were still incorrect. Burling was able to play a type of translation game with his son at this stage, in which Stephen would supply the Garo equivalent of an English word or vice versa.

The bilingualism displayed by Stephen is ascribed by his father to the influence of his Garo caregiver who brought him into contact with other Garo speakers, even on their stay in Gauhati which is outside the Garo Hills. Here (1959:62) Stephen showed considerable shyness when in the company of non-Garo speakers. On their return to the Garo Hills, Stephen spent much more time in the company of his mother, which resulted "in an explosive expansion of his ability in English"(1959:62). The tendency shown by Stephen to seek out the company of someone who could speak a language he understood, says much for the process of language development. It highlights the importance of the linguistic environment for a child who is, at an obviously receptive age, responding to a language which he hears consistently, in normal usage. More than this, the influence of linguistic environment (and the child's sensitivity thereto) raises questions about language tests

that are given to children on a one-off or random basis in strange environments. Whereas the method of naturalistic observation is able to take account of changes in the routine of the child and his responses to these changes, the experimental research mode is far less sensitive to the effect which the changed environment has on the language of the children being tested.

One of the purported advantages of an experimental test of language development is that it enables some general hypothesis to be tested with a reasonably large number of children. Such tests can be replicated by other researchers to confirm or disconfirm the conclusions reached previously. In other words, it allows for a large number of comparisons to be made among children with respect to language development. With naturalistic research, direct comparisons of this type are lacking; however, there is still the possibility of some fruitful comparisons between research findings. Burling is able to make useful comparisons between the phonetic development of his son and the patterns of such development suggested by earlier researchers, notably Jakobson and Halle, and Velten (1959:64). Burling is also able to use his findings to disagree with Leopold on certain of the latter's views on child language, in particular Leopold's view that "syntax comes before morphology" (1959:65). Burling found that in Stephen's development of Garo, both syntax and morphology developed simultaneously (1959:66). Despite the fact that Stephen

borrowed a number of English morphemes into Garo, "there was seldom any question as to which language he was using since affix morphology and syntax were either all Garo or all English"(1959:67).

In summary, it can be said that Burling's account of his son's language development is a perceptive one. It is not at all comprehensive, since it has phonetics and syntax as its focus - more than 60% of his article is devoted to these topics - and only three paragraphs are given to semantics. However, he has rendered a detailed account of the topics which appeared to be of primary interest to him, and he has tested the theories of other researchers, by comparing his observations with their predictions. More importantly, he has conducted his observations in the natural context of his son's growth, with interesting comments on the effect of Stephen's mother's presence (or absence) on the child's language development. This factor is a potentially crucial one for the study of language development, with mothers in modern times tending to go out to work more, and leaving their young children in the care of others. Experimental studies are not able to delve into this area of research in anywhere near the same depth as a continuous naturalistic study.

Brown et al, in Bar-Adon and Leopold (1971), describe a longitudinal study of three children, Eve, Adam and Sarah over differing stages of language development. The interesting feature of this research was the simultaneous

study of the respective parental utterances which had been addressed to these children. Although this study was primarily syntactic, and was done in the aftermath of Chomsky's early published work on syntax (Chomsky 1957,1965), the consequences for research method into child language as a whole are worthy of consideration. The authors stress that in any grammatical study the role of interpretation is crucial : the mere recording of spontaneous speech is only the first step towards knowledge, a knowledge which "is worked out from spontaneous speech by a process of inference that is far from being mechanical or certainly correct. The process is something like trying to fit together the pieces of an immense jigsaw puzzle ... and not at all like the process of doing experiments in a psychological laboratory" (1971:384).

This latter point is a telling one : the authors are saying that knowledge in the realm of grammar (which is one aspect of language) is obtained by analysis of a wide variety of details, and by attempting to find links between different sections of a corpus of data. Such knowledge is not obtained by attempting to control the variables in a language situation as one would try to control variables in a psychological (or other scientific) type of experiment. There is no straightforward application of method, result and conclusion to this realm of knowledge; instead there is a painstaking process of inference based on the consideration of data. It would be appropriate to extend

these authors' argument and to claim that the data for analysis, if it is to be used to construct a theory of (natural) language, should be obtained from the study of children in naturally-occurring circumstances. It is difficult to argue for the extension of experimental methods of studying language development if these methods exclude the possibility of linguistic naturalness.

Another well-annotated study of language development using the longitudinal naturalistic method is that of Kaper (1985). Kaper kept records of the speech of his two sons, Erik (E) and Hans (H), for approximately six years in the case of Erik (1;11 to 8;2) and just over four years in the case of Hans (1;7 to 6;0). The interesting aspect of Kaper's research is that he concentrated on recording utterances which deviated from standard adult use. In fact, Kaper stresses (1985:4) that in his opinion, "there is no essential difference between child language and adult language and that the proper method of studying "child" language is to compare it frequently with "adult" language. Also interesting from the point of view of this thesis is that Kaper devotes considerable space to a discussion of semantic development. The context of the study is Rotterdam and the language used by E and H is Dutch.

Kaper addresses one of the interesting problems in the study of semantic development, viz. to what extent do children acquire a language by imitating adult speech and to what extent do they form their own hypotheses about the

target language. He mentions the individual differences between his own sons E and H in their attempts to acquire Dutch. H used to "parrot nearly all the words he heard (also when not addressed to him), without paying attention to the meaning. E never did, on the contrary, he refused to repeat words when he was prompted to do so ... Apparently he did not speak the words before he felt sure of the right pronunciation" (1985:8). This observation has important consequences for experimental work on child language; if correct, it may explain in part why certain children are unresponsive in a test situation. Reticence may not reflect a lack of competence; on the contrary, a child who is trying to speak in close conformity with adult speech may simply be holding back certain words or phrases until he feels confident about uttering them.

Kaper relates his observations on the semantic development of his son (E) to those of other researchers. In the chapter entitled Container and contents he describes E's use of the morpheme op to express absence as well as to refer to an empty container. He explains E's "deviant" use of op by saying that, to a child, this component could be taken to mean either the food or drink which had been in the now empty container or the empty container itself. For Kaper, "in the child's mind the reference is rather vague", and he (Kaper) refers to the analogy of "semantic fog" used by other researchers (Leopold and Barrett) and also the view of Vygotsky on the child's thought processes (1985:11).

Kaper also notes that both his sons used the word leeg (empty) synonymously with the sense of op in Standard Dutch, "expressing that the contents of a container are finished" (1985:11). However, his son H appeared to use the word vol (the antonym of leeg) to refer to the contents, and not the container (1985:12) and (1985:Examples;1). However, as H grew older he was able to distinguish between reference to container and reference to contents. Kaper mentions that H jokingly took some chocolates out of their dish and handed them over, when asked for "the chocolates" (1985:12). Kaper cites the example of the phrase "the matches" which in normal adult usage refers to the container plus its contents (1985:12). In passing, Kaper mentions the deviant use of the plurals of mass nouns by his children but notes also his own deviant use of the plural tandapasta's (toothpastes) in conversation with his wife (1985:12). This observation accords with his general argument that child language has no separate existence - it is, for Kaper, essentially the same as adult language.

In discussing his children's use of adverbs of time, Kaper (1985:14) finds that both E and H use gister(en) (yesterday) to refer to the past, but not necessarily to the previous day. Although Kaper does not mention it here, adult speakers also use phrases in which time reference is inexact. In South African English the phrases just now and the other day are cases in point : the former could be used to refer to an event which occurred up to an hour ago, while

the latter could refer to an event which occurred up to a week prior to the time of speaking. Although his children use gister(en) in an inexact way, it is only the degree of inexactitude that is apparent. Like their deviant use of container words, it is this vagueness which shows the incomplete development of their semantic systems. However, it is interesting to note that adults also use phrases having vague reference, so that this "deviance" appears in language across a spectrum of ages. Once again, Kaper compares his observations with those of other investigators, and although he acknowledges that his are not new, they "may contribute to extending the material already collected by others" (1985:15). This statement underscores the strength of the method of naturalistic observation of language development : knowledge accrues from the cumulative effect of the observations of different observers. Similar observations are thus able to be taken as being mutually confirmatory. In experimental tests of language development one also finds similar observations, but these are not observations made about naturally-occurring language, and thereby lack the generality of the naturalistic studies.

Kaper (1985) has an interesting chapter on the antonymous use of words by children. By this he means the use of a word by a child in a situation in which an adult would use the antonym of that word, e.g. using the word hot when touching a cold object. Kaper noticed this use in a child as young as 1;7. He also records the antonymous use

of deictics like compounds of komen (come) and gaan (go). However, Kaper mentions that children frequently use words antonymously as well as correctly and, in fact, he is not able to cite a word which he observed a child to use solely in an antonymous way. By regular observation of his own children Kaper is able to discern patterns in their antonymous use of words, as when, E uses zoeken ('look for') meaning vinden ('find'), but also uses vinden in appropriate contexts (1985:55). Nine days later, Kaper notices that E uses zoeken to mean"'meeting a person at the railway station'" (1985:55). Although this use is idiosyncratic, Kaper is able to make comparisons between the speech of E and Leopold's daughter Hildegard (1985:56). In E's case, he uses vinden in "Zal ik hem vinden" whereas Hildegard (speaking in German) says "Soll ich's mal finden". Both sentences have the English equivalent Shall I find it? Here, once again, naturalistic observations are a fruitful source of comparison in the study of semantic development.

Kaper uses his observations and those of other researchers to frame two hypotheses about the use of antonymous words by children (and, as he also points out, by adults). His first hypothesis is that speakers, when searching for a particular word, may latch on to its opposite because somehow the antonym is associated with the word being sought : "It is as if they are scanning a semantic field in which among other things words are associated with each other because they are opposites"

(1985:62). His second hypothesis is that "in children the discrimination of word-meanings is initially very hazy : the 'semantic fog' .... clears away in the course of time" (1985:64). In fact Kaper concedes that semantic development may never be taken as complete, but should rather be viewed as a "continuous process" (1985:64), continuing into adulthood.

On reflection, Kaper's studies are very valuable, even though he concentrates on two children. He points out important similarities between the speech of his children and that of other children in the literature. He also shows that many of the deviant forms which children use are analogous to errors which adults make and are, in any event, deviant only insofar as they are not in line with the norm of the particular language. They do, nevertheless, reveal that the children are "making use of a potentiality inherent in the language" (1985:101). Their errors are therefore not non-language but merely "incorrect" language. The form used by the child can usually be traced back "to a potential form in his mother tongue" (1985:101). This adds weight to his argument that child language is a language which does not exist in its own right - it is, in essence, no different from adult language.

Kaper's study of the language development of his sons is of a different kind to the studies of Halliday and Burling discussed earlier in this chapter. However, where Kaper has said little about research method, he has recorded

a commendable number of example sentences - 419 - in the appendix to his book. These example sentences are grouped thematically and the exact age of the child concerned in each case is given in years, months and days. Like Burling, he has compared his observations with those of other students of child language, and he has also given much contextual information. Although he has not written a grammar of child language, as Halliday did, he shares Halliday's child-centred approach and is able to display great empathy for children in his interpretation of their utterances. All points considered, Kaper has made a significant contribution to knowledge in respect of semantic development.

It is regrettable that these excellent naturalistic studies of language development, with their unique insights into the way in which children use language, were not published earlier, so that the advantages of their methods could have become apparent sooner. It is still not too late for other researchers to take note of these advantages and to incorporate as many as possible in future research. What is apparent from reflection on the research discussed in this chapter is the powerful influence of the child's environment - both linguistic and extra-linguistic - on his language development. It is this total environment, with the watershed moments it contains for language development, which are accessible to the naturalistic researcher. Those involved in experimental studies can catch little more than

glimpses thereof.

In the concluding chapter of this thesis, consideration will be given to ways in which the gulf between naturalistic and experimental studies can be narrowed. This question is naturally a problematic one; access to children for sufficient periods of time being the greatest difficulty. However, if child language studies are to be improved, it seems clear that those using exclusively experimental means will need to modify their methods.

In the concluding chapter, attention will be given to the question of the study of semantic development in general. Researchers need to place this study within the perspective of language study as a whole, and, in particular, to assess the profitability of the study of semantic development (of children) in isolation from the study of semantics.

Chapter 7

CONCLUSION

This thesis set out to analyse the existing research methods in language development studies, with particular reference to semantic development. In the course of this thesis, other aspects of the study of language development have been discussed. In this concluding chapter, an attempt will be made to draw together the strands of the various arguments and to indicate the importance of these arguments to the study of language development as well as to the study of semantics.

This chapter will be divided into five parts, as follows:

1. Analysis of methods used in experimental studies of language development.
2. Analysis of methods used in naturalistic studies of language development.
3. Comparison of the methods discussed in parts 1 and 2 of this chapter.
4. Recommendations for future research into language development, with respect to research method.
5. Implications of the findings of this thesis for the study of semantics.

1. Analysis of methods used in experimental studies of language development

In the second chapter of this thesis, there was a discussion of the artificial nature of the situations in which tests of language development are administered. There are many facets to this

artificiality but it is as well to consider a general point. Language is a natural phenomenon and develops in humans in the course of the natural overall development of the speaker. It is a phenomenon which not only occurs *in* context but which is also *bound to* context. Tests of language development do more than test the language of a child - they actually involve the creation of a context for the language which is being tested. The central question is whether, within the boundaries of this non-natural context, the language being used by the testee can be viewed as natural and thus as representative of the language which he uses in other, (i.e. the majority of), situations. In short, are experimental tests of language development valid?

In Chapter Two, the factors which tend to invalidate the results of these tests were discussed in detail. There would appear to be a number of physical and psychological factors which affect the results of these tests. Chief among the physical factors are the strange apparatus (tape-recorders, videotape cameras and even notebooks) which interpose themselves between the interviewer and the child. In a sense, these instruments become silent participants in the test itself, and as such may prove to be a source of distraction from the language focus of the test. Then there is the interviewer, often a stranger to the

child, and definitely someone with whom the child ought to feel a rapport before the latter will begin to speak at all, let alone in a natural way. If tests of language development are to show even a marginal validity, then the interviewer must at least allow for a reasonable period of familiarisation between himself and the child being tested. This factor will be discussed again in more detail.

One psychological factor is the anxiety level of the child in the test situation, which may be related to the demands which he perceives to be inherent in the test. (This factor will no doubt vary from one age-group to another). Tests of long duration tend to induce boredom as well as fatigue in child subjects, especially in the case of younger children. There would seem to be no standardisation of test duration with respect to the age of child subjects. Having regard to the boredom/fatigue factor it would seem desirable to impose a reasonable upper time limit on tests, at each age.

If the interviewer is male, this factor, too, could influence the results of the test, since most children of the ages often being tested are more accustomed to hearing their mother's voice than that of anyone else. Indeed, the absence of their mother from the test situation may be a disturbing factor for very young children.

Reference was made in Chapter Two to the Observer's Paradox which has an influence on the outcome of language tests. In the case of children in particular, the relationship between interviewer and subjects is a delicate one. There is insufficient knowledge as to the extent of the influence of a non-maternal observer on the performance of children in language tests. Up to now the possible negative effects of an outside observer on test outcomes appear to have been overlooked to a large extent. Elliot (1981) fails to mention the drawbacks of experimental studies of linguistic development in the chapter of her book, Child Language, dealing with this topic. The closest she comes to a cautionary note is to state that an experimental study attempts to probe "the natural course of development, without intending to interfere with this development in any way" (1981:110). It remains the contention of this thesis that the mere presence of a strange observer detracts from the naturalness of an interview situation, and thus also diminishes the possibility of obtaining natural linguistic data in such a situation.

In Chapter Three, there was an analysis of the materials used in a number of tests of language development. The main point being made in that chapter was that materials which are artificial and often (very poor) models of real objects, may be unsuitable for use

in tests of semantic development. In particular, the rows of garage-like structures, specially designed for use in a test situation by Donaldson and McGarrigle (1974), bear little resemblance to the objects denoted by the lexical item garages. Since the objects used in the test were not real garages, the authors are not consistent in their use of terminology, and use words like garages and garage spaces interchangeably.

Similarly, the test designed by Charney (1979) to study deictic comprehension, in which a number of means of transport are used in an unusual context, has a number of artificial aspects. The test seeks to probe the understanding of perspective, yet the method of using objects whose size (as models) is out of proportion to their real dimensions is distinctly odd. An inaccuracy in research design such as the use of different distances of object models from the participants in the test (when such distances ought to be equal) is also a feature which tends to weaken test validity. It appears that, in tests which are constructed to test linguistic development, researchers tend to concentrate too much on the language aspect alone, and neglect the considerations of artificiality or inaccuracy in the physical conditions which they impose on the test.

The use of toys and models in general as part of tests of semantic development is questionable. As a

yardstick of semantic development, that is, as a measure of how advanced children are towards achieving semantic competence, the use of tests involving toys and models seems inappropriate. By referring to toy dogs as dogs or to garage models as garages we are, as adults, not indulging in everyday adult speech. Although, as adults, we are aware of the distinction between models and the objects which they represent, it is not necessarily the case that children in general perceive this distinction in a similar way. In order to obtain a more accurate impression of children's semantic development towards (adult) competence, it would be preferable to design tests involving real animals or real garages.

The fourth chapter of this thesis deals with the type of language used in studies of semantic development, and the use of nonsense syllables (NS) in particular. A criticism similar to that mentioned in the previous paragraph is pertinent here. Whereas adults generally recognise NS as such, children may react with confusion when confronted with NS. The whole question of what is real for the adult versus what is real for the child looms large in this type of test. Since adults do not normally use NS in everyday speech, it seems out of place for NS to be used in tests which probe children's progress towards semantic competence.

In respect of tests of semantic anomaly, the use of polar options is not a reflection of reality, with its subtle gradations. Children who perceive such subtleties ought to be given choices of ways in which the express them. It is also quite possible for experimenters to overlook alternative (equally plausible) interpretations of sentences in these tests. Once again, the respective emphases of perception of adults and children need to be recognised and studied. It would appear that a valid, "age-free" test of semantic anomaly is a difficult aim to achieve.

In Chapter Five, there was a critical discussion of research methods used in the study of relative clause acquisition. In the course of that chapter it was emphasised that, in the design of test sentences, various Gricean conversational submaxims should be observed, particularly those of clarity, brevity and orderliness. An important criticism of the tests mentioned in that chapter is that they are often based on unrealistic situations, such as making toy pigs jump over toy lions. It was also pointed out that elicitation of relative clauses is difficult to achieve. The general point was also made that researchers, often keen to quantify their results, tend to overlook individual differences between children. It is these differences, and the reasons which give rise to them, that are a fertile area for further

research. Later in this chapter there will be a summary of the main criticisms of experimental tests of language development, and tests of semantic development in particular.

2. Analysis of methods used in naturalistic studies of language development

In Chapter Six, a number of naturalistic studies of semantic development were discussed. The work of Halliday, in particular, in studying the developing language of his son, has provided a sound alternative to the plethora of experimental studies of LD. Halliday was able to detect and describe trends in his son's LD because of the intensive yet relatively unobtrusive manner in which he went about his work. Halliday also demonstrated an ability to systematise his observations and to relate groups of observations made at different stages of his son's development.

By studying his son in naturally-occurring situations over a relatively long period, Halliday was able to present a truly developmental account. He was able to distinguish the relevant and the consistent from the purely random elements in the corpus of his son's vocalisations. Halliday's study was path-breaking in the sense that he attempted to portray a child-centred perspective on language. He was able to take this approach because of the long periods of careful research which he undertook. However, Halliday

was also careful enough to provide, in his theoretical framework, for a transition from the child-centred perspective on language to an adult-centred one, i.e. linguistic competence.

Specifically, with regard to semantic development, Halliday describes how the child becomes aware of the functions of language, and is able to exploit the potential in language to convey meaning. Halliday also refers to the need, in the child's linguistic system, for there to be a systematic relation between the content and the expression. Because he was able to observe his son continually in similar situations, Halliday was able to isolate this aspect of his son's developing language.

One of the most cogent reasons for acknowledging Halliday's contribution to language development research as an effective one, is that he linked LD closely in his theory with the growth of the child as a social being. The sociolinguistic dimension of Halliday's work is fascinating. In constructing this aspect of his theory, he refers to the work of a number of other researchers in the field of sociolinguistics. It would be interesting to compile a number of reports of naturalistic observation of semantic development made in the Halliday 'mould'. Whatever the shortcomings of his theories may be, it is correct to hold that his overall approach and painstaking

observations set a standard for future students in the field.

Burling's account of his observations of his son's LD is different from that of Halliday both in its nature and in its scope. Burling's son experienced a relatively disrupted early childhood, with periods of enforced absence from his mother and with changing degrees of exposure to English and Garo in the first 3 1/2 years of his life. Burling documents the effects of these changes on the LD of his son, and also gives an account of his son's language during sleep. Burling mentions some of the difficulties inherent in the study of child language, chiefly the paucity of the material, or speech corpus, which the researcher is able to record. He also notes stages in his son's semantic development, particularly with respect to the use of colour terms, spatial terms and to metalinguistic awareness. Having been able to study his son's LD in a natural context for about two years, Burling is in a strong position to see this development in an integrated way and to note, in particular, the influence of important environmental factors.

Brown et al, in their important case study of three children, come out strongly in support of the importance of inference and interpretation in the study of child language. The type of analysis of the speech of children which these authors advocate is essentially

one which lacks experimental controls, but one in which a wide-ranging study of spontaneous speech is given prominence.

Finally, Kaper, in his longitudinal research into the language of his sons, puts forward the tenet that child and adult language are essentially similar. Kaper makes extensive use of so-called "deviant" forms in child language to further his argument that this language is far closer to adult language than many theorists would concede. He maintains that many adults exhibit linguistic deviations which do not differ much from those which children reveal. From the point of view of semantic development, Kaper's research is interesting in that it highlights aspects such as the usage of temporal adverbs and container words as well as dwelling on the antonymous use of words by his children. Kaper's research is extremely thorough and well-documented. He has taken pains to compare his findings with those of other researchers, and thus succeeds in making his central argument stronger. Above all, like the other researchers already mentioned in this chapter, his focus is sharply on the child, and on the child's semantic development in the context of a broader social growth. It is once again this holistic approach to the study of child language which provides linkages between various stages of development as well as between the child's milieu and his language *within*

that milieu.

3. Comparison of the methods discussed in parts 1 and 2 of this chapter.

Experimental studies of language development have the advantages of being of relatively short duration and of ensuring that, for comparative purposes, language data from a large number of children are gathered. Since such studies often focus on a small area of language, and can also be designed to incorporate statistical tests, their popularity among researchers has been considerable. Many reports based on such studies have appeared in reputable journals, especially the JCL. Quite frequently, published articles on various aspects of language development (in the experimental mode) have given rise to profitable debates among researchers. In this sense, at least, these articles have been of use : their publication has led to experimental researchers in different countries being able to compare their findings. The resulting incremental growth of knowledge in the field of language used by children in test situations has been considerable. But the point here is that such knowledge is essentially about language *used in specific situations*; situations which are not entirely natural. The main purpose of this thesis has been to argue that, because of their artificiality and because of certain inherent defects of method, experimental

tests of language development are, in general, poor yardsticks of LD.

The main criticisms of experimental studies of LD are the following:

1. The setting of these tests is an artificial one; thus no valid conclusions can be reached regarding language development in a natural situation.

2. The use of strange apparatus by those involved in testing the children may distract the subjects or even produce anxiety.

3. The mere presence of an observer serves to distort the results of such experiments.

4. The duration of experiments can tend to produce boredom as well as exhaustion among subjects, resulting in stereotyped response patterns or even a lack of response.

5. It appears that certain children choose to remain silent in an experimental situation rather than reveal what they may consider to be ignorance. This silence may itself be interpreted as a sign of ignorance by the interviewer.

6. The materials used in some language development tests are at times poor models of the objects which they are intended to represent. This factor, coupled with the use of toys in tests of semantic development in particular, can lead to errors in reference.

7. The language used in the construction of test items in experimental studies, and particularly the use of nonsense syllables, is often inappropriate and potentially confusing.

Taken collectively, the disadvantages of experimental studies of language development appear to outweigh the advantages. In the opening chapter of this thesis, Chomsky was cited as classifying linguistics as part of cognitive psychology. Indeed,

in studying LD, one is conscious that only a part of the child's behaviour is under scrutiny. Language, however, meshes closely with other aspects of human behaviour, and it is questionable whether language per se can be abstracted from the continuum of such behaviour for the purposes of research. This is essentially what experimental researchers are trying to do.

A number of studies discussed in this thesis involve the child in activities which test factors other than language, such as memory and imagination. Linguistic competence is closely linked with the ability to recall, as well as with creativity. It is important that experimenters be aware of these links when proceeding to draw conclusions from their research. Semantic development is but one of the many facets of child development. The growth of the child in associated areas of cognition should be taken into account when any assessment of semantic development is made. The overriding caveat to experimental researchers is to bear in mind that language is integrated into behaviour as a whole and cannot be isolated artificially for research purposes.

In the case of naturalistic studies of LD, the main benefit is that the research takes place in a natural situation. Any conclusions which may be drawn from such studies are conclusions about normal language

i.e. language as it occurs in everyday situations. The researcher or observer also enjoys the advantage of becoming relatively well-acquainted with the child. This knowledge of all aspects of the child is invaluable in forming judgments as to the degree of his language development. Furthermore, the value of keeping records of the language of the child in two or more similar situations over a period of time is most important. Such observations provide the type of natural check on the precision of the data which a researcher obviously requires.

Naturalistic studies have the further advantage of enabling data to be recorded at unusual times, such as when the child is talking in his sleep. Naturalistic studies also allow for important connections to be made between dramatic changes in the tempo of a child's LD and highly significant non-linguistic influences, such as separation from, or reunion with, a parent. Naturalistic studies are better able to relate LD to other important aspects of development, like memory and imagination. Knowledge of a child's overall social and cognitive development provides an essential framework for the study of LD. Naturalistic studies are able to achieve this overall perspective on child language. A far more holistic approach to the study of LD is achieved via naturalistic methods than by experimental ones.

Naturalistic methods are, however, not without certain drawbacks. They are usually very time-consuming and make great demands on the energy of the researcher. The sheer volume of data which the child presents to the researcher can tend to become unwieldy and hence difficult to analyse. As Elliot (1981) correctly mentions:

"Thus we face the major frustration of the study of child language - just as the child begins to make big advances he becomes so voluble and his language so varied that we have to turn to other means of collecting and organising our data" (1981:102).

In many instances of naturalistic observation of LD, the research is carried out by a parent of the child in question. This is not wholly desirable since a parent cannot be expected to be an impartial observer; on the other hand, a parent is usually dedicated to this type of research and will thus persevere in his or her efforts. Another disadvantage of naturalistic studies is that they are often difficult to replicate. The age at which one child shows certain signs of development often differs considerably in the case of another child. The time available for the study of the LD of different children also varies. Thus it is unlikely that any two researchers would be able to confine their studies to children at the same (or a similar) stage of LD, for

the same period of time.

Elliot (1981:102) explains that, when LD becomes rapid and is spread across a number of aspects of language at once, researchers become selective and tend to concentrate on specific areas. It is then that, Elliot claims, "most turn to experiments instead of naturalistic studies" (1981:102). If it is the burgeoning growth in the amount and range of the child's language *alone* that inclines researchers towards experimental methods, this is very unfortunate. It would seem that, in keeping with the general drift of this thesis, at this very stage more, and not less, reliance should be placed on naturalistic methods of research. Without doubt, this is difficult to achieve, yet it seems most desirable and correct as a means of research.

Research trends being what they are and bearing in mind that there will thus be a disinclination on the part of researchers into child language to avoid the use of experimental studies, despite their disadvantages as set out in this thesis, it is appropriate to examine ways in which such studies could be designed in order to achieve results of greater validity. At the same time, there will be an attempt to investigate ways of promoting more effective studies of language development in the naturalistic mode. These matters will now be discussed.

4. Recommendations for future research into language development, with respect to research method.

In order for valid results to be obtained in any study of language development, it is the contention of this thesis that the language being studied should be natural in the sense that the child should not be placed in artificial circumstances for the purposes of research. If the child is to be 'interviewed', then the approach should be as relaxed and as natural as possible. Audio-visual recording devices should be completely unobtrusive, and, if the interviewer needs to make notes, this should be done in a discreet manner.

It is important that the child should feel at ease and not under pressure to 'perform well' for the sake of the interviewer. The reassuring approach employed by Armbuster (cf. Chapter Six) is to be recommended in this regard.

The duration of the period of such an 'interview' is important : it generally depends on the age of the child(ren) in question, but flexibility is crucial. Elliot (1981:110) mentions a period of "about fifteen minutes". Periods longer than this lead to boredom or fatigue for many children, besides being of longer than normal duration in terms of what could be expected for most types of adult-child interaction. More importantly, interviews which are long tend to be

repetitive and thus artificial in their structure. It is preferable to keep the period short and extend it only if it is clear that the child is responding in a natural and reasonably fluent manner. Even for older children it is recommended that such interviews should not last more than fifteen minutes. It is better to stop while the child is still interested and enjoying the interaction. It should be stressed, however, that such enjoyment should not be taken to mean excitement or obvious boisterousness. The child should not view the entire process as a game, but rather as a component of a normal conversation.

Above all, the language used in interviews should be natural, lucid and economical. The use of "nonsense-words" is questionable as a method of research into LD. Furthermore, if objects or props are used in such interviews, care should be taken to ensure that these, too, are natural. If toys are used then the researcher should refer to them as toys (or as models) but not as the objects they represent. In all cases, the language used and the situations chosen should be as natural as possible.

The researcher should concentrate on the linguistic analysis of the data, and not on obtaining a corpus from a large number of children. Emphasis should lie on the quality of the language data, not on mere numbers of children interviewed. By giving

attention to smaller numbers of children for shorter periods of time, the researcher will be able to learn more about the LD of the child subjects by following up on initial interviews after a period of analysis of the data. In this way the research method will begin to approximate to that of the naturalistic mode. Its validity will therefore increase as well. Seen purely from a statistical viewpoint, large samples are desirable in an experimental test situation. However, it would appear from a reading of a number of articles in research journals, that the statistical facet is being overplayed to the detriment of linguistic analysis.

The recommendation of this thesis is that more care should be given to ensuring that the data itself is valid, by capturing such data in a natural situation, rather than giving undue attention to statistical requirements.

The number of researchers employing naturalistic methods is no doubt very small. Nevertheless, such methods (although time-consuming) yield valid insights into the process of LD. In order for these insights to be made available to other researchers, it would seem desirable that an information network be set up among 'naturalistic' researchers. In this way, similar findings in the field could be used for purposes of corroboration. It is also likely that a more co-

ordinated research effort would result from such a research network, leading to a sharper focus on particular areas of research. No doubt the relative isolation of researchers using naturalistic methods has led to their studies becoming diffuse. It would be interesting to examine the effect of attempting to fuse the various strands of research in the naturalistic mode.

Another recommendation that is directed to 'naturalistic' researchers is that they should provide more detailed research records, showing the exact ages at which their subjects made important utterances or achieved apparent 'milestones' in LD. These records should be specific as to the age in years, months and days of the child(ren) being studied. The advent of the micro-computer has made possible the sorting of a large quantity of information. One of the sorting devices available is that of collation according to age. This approach ought to be valuable, especially when used to compare one's findings to those of others. Another useful method of sorting is to collect utterances of a similar kind made at different ages. Examples of this are found at the end of Kaper (1985). Even Kaper's method of data presentation could be improved by a sub-division into more refined categories, e.g. locatives, temporal adverbs, container words and antonyms. In this respect, the detailed

index of research articles given in the JCL, Volume 12 has proved extremely useful for the purposes of the current thesis.

A more general comment, therefore, directed at research into LD, would be that there is currently a lack of organisation of research findings in the literature. There has been an explosion of *information* in recent years without a commensurate increase in *knowledge*. It is suggested that inadequate filing and sorting of information is one of the key reasons for this disparity. Just as researchers need to organise and analyse a plethora of data which child subjects present, so is there a need for the findings of individual researchers to be gathered and sorted into useful categories. In this way, being able to distinguish the salient similarities among various findings, future researchers ought to be able to consolidate the work of their predecessors.

5. Implications of the findings of this thesis for the study of semantics

The main conclusions and recommendations of this thesis, as set out in the first four parts of this chapter, require a final perspective. They need to be seen in relation to the study of semantics as a whole. The concluding section of this thesis will therefore contain remarks on the implications of these findings for the study of semantics.

Some of the more important questions to be addressed are the following:

1. To what extent does the study of semantics in child language differ from the study of semantics in adult language?
2. Bearing in mind the point raised in question 1 above, is it valid to make a distinction between child language and adult language?
3. Is there such a thing (entity) as semantic competence?
4. To what extent is the study of semantics, in general, a profitable one?

The above questions are interdependent, so that, rather than attempting to answer them separately, it will be attempted to address them holistically.

In the introduction to The Articulate Mammal, Aitchison (1976:13) submits that "child language" overlaps with the "language of normal adults" : she does so by means of the following series of diagrams:

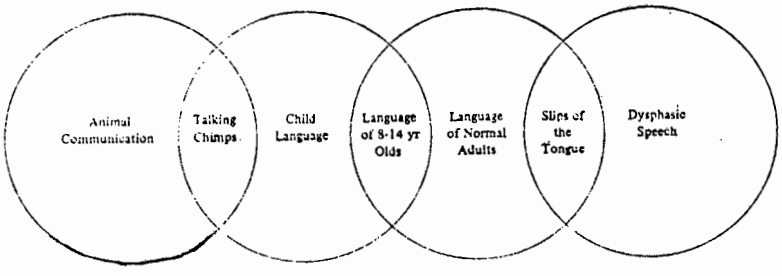

Fig. 4 (1976:13)

The area of overlap of the two central sets is designated as the "language of 8-14 year olds". This implies a gradation : a phasing-out of child language and a phasing-in of adult language. This model seems

more appropriate than one which views the change from child to adult language as an overnight, radical one. However, it ought to be stressed that, despite differences in external forms, child and adult language are more alike than different. This point was made by Kaper and is one that is endorsed by the author. Both child and adult are members of the species homo sapiens and ought therefore to possess language in the broader sense, as defined by Hockett (1963) and by Aitchison (1976:36-43). It would therefore seem more appropriate to refer to child language as 'developing language' : it is not the same (or not *quite* the same) as the language used by adults because children are biologically and in terms of experience, less *developed* than adults.

Phonetic and phonological development is more clearly defined and more rapid than semantic development (cf. Menyuk (1971) for evidence in support of this statement). A similar comparison can be drawn between syntactic and semantic development. That this should be so is a corollary of the fact that the number of permissible sounds and sentence contructions in a language are *both* far fewer than the number of possible *words* (and hence potential meanings) in that language. Semantics is thus a far more problematic area of study than either phonology or syntax. Semantics is further complicated by the ongoing debate concerning certain

key concepts in the subject, such as meaning, reference, singular terms and proper names.

Disagreement among linguists in the latter respects makes the study of semantics in relation to developing language correspondingly more difficult : not only is it unclear *what* children mean; it is also in dispute as to whether it is possible to ascertain what they mean, or what adults mean, for that matter.

A fertile research area could well be the study of differences in the degree of metalinguistic awareness exhibited by adults and children. One of the time scales for the development of metalinguistic awareness cited in the literature (Tunmer et al, 1984:28) is "the period from approximately 4 to 8 years of age". If children's linguistic development bears a definite relationship to their development of metalinguistic awareness, then researchers studying the semantic development of children ought to be able to obtain a more accurate idea of this developmental aspect by comparing it with the extent to which there is an awareness of language as an object of thought.

One might then attempt a rather broad (or general) developmental classification of language as follows:

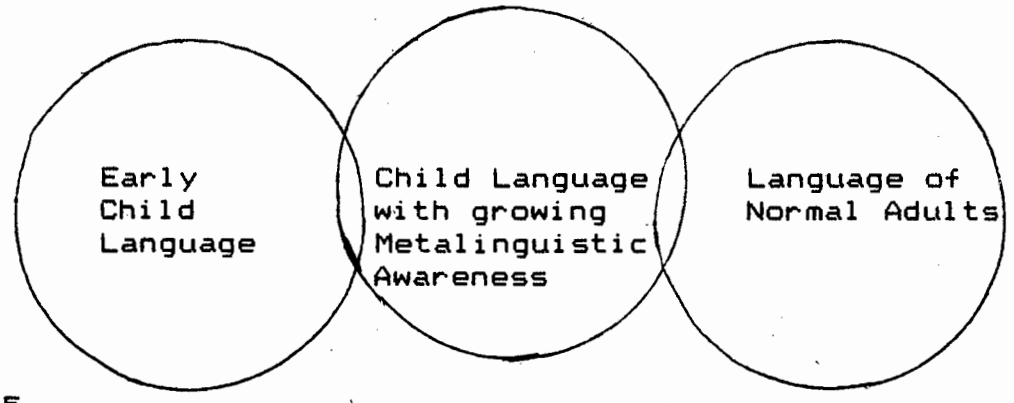

Fig. 5

The first set would correspond to children aged zero to six and the second, to children aged four to fourteen. One of the main points to consider in the study of the language of children, therefore, is the extent, or degree, of their awareness of language as an object of thought. The degree of metalinguistic awareness of a child subject would appear to have a material influence on the way in which he 'performs' in a language test. A child who is able to think deliberately about concepts like synonymity or ambiguity, for instance, would appear to possess a greater degree of semantic competence than one who is less aware of these concepts. However, a similar criterion could be applied to adult subjects.

It also appears reasonable to assume that certain adults possess clearer ideas of concepts like ambiguity and synonymity than others. Certain adults are said to 'possess a good vocabulary' because they are able to express their thoughts more precisely than their fellows, using a wider range of lexical items. Such an ability is not necessarily advantageous in all situations : its merit would appear to be the greatest

when these adults are in the company of others of like ability.

Nevertheless, semantic competence is not an absolute concept : users of a language are able to acquire new lexical items throughout their lifetimes, as well as being able to improve on their choice of words in a particular communicative situation. In the case of children acquiring a language, and thus moving gradually towards (some sort of) semantic competence, the key problem is the meshing of concepts with linguistic structures. As Schlesinger (1981:239) puts it, "the child must find out how the multi-dimensional cognitive space is mapped into language-specific semantic categories". This is a broader, or more general, description of the process to which Slobin (1981:197) refers as the pairing of "prototypical event and canonical form".

For the adult,the greater number of events are no longer prototypical, and linguistic forms to describe these events are relatively stereotyped. In this respect, the language of adults is more developed than that of children. However, novel events (e.g.the discovery of a new technology like laser optics) result in adults having to link these (prototypical) events with new lexical items. Unlike the child, the adult is able to slot the new concepts (and new lexical items) into a conceptual-linguistic structure which is already

relatively sophisticated.

Initially, some adults experience more problems than others in coping with what is essentially a re-structuring of their systems of concepts and language. But the 'elasticity' of the semantic component of language ensures that such changes are accommodated. Moreover, the reality and the usefulness of the changes is an incentive for their accomplishment. The study of how such 'innovations' enter adult linguistic systems would appear to be a fruitful one, especially in the light of the concept of "future shock". An improved semantic analysis of the plethora of new lexical items that enter language via changing technology and life-styles is not likely to cushion the mental discomfort which often accompanies such changes. What it may achieve, however, is an improved insight into human mechanisms of coping with change.

In a sense, this study is similar to the study of how children, in a relatively short time, absorb a vast number of lexical items into their developing language. Both child and adult are faced with changes in their environment. Changes in language mirror environmental changes as well as human reactions thereto. In the final analysis, there seems to be much merit in viewing child and adult language as essentially similar. Semantic development can thus be viewed as a component of overall human development. The study of the

developing language of children ought thus to shed light on the (still) developing language of adults. There will always be an area of overlap in the study of the semantics of the language of members of different age groups.

Although the subject of semantics is opaque and presents its students with a series of controversies, it is nevertheless worthy of study for it enlightens our understanding of humanity. Humans, whether children or adults, use language to mean. Precisely because people change in response to both internal and external forces, their language, and hence what they mean, will remain a dynamic entity. For this reason alone, the study of semantics should be pursued. The study of semantic *development* is an inalienable and important part of this overall pursuit.

REFERENCES

Aitchison, J. 1976. *The Articulate Mammal*. London: Hutchinson.

Andersen, E.S. 1975. 'Cups and glasses: learning that boundaries are vague'. *Journal of Child Language* 2, 79-103.

Angiolillo, C.J. & Goldin-Meadow, S. 1982. 'Experimental evidence for agent-patient categories in child language'. *Journal of Child Language* 9, 627-43.

Armbuster, T. 1981. 'How to influence responses to "Is Kathy easy to see?"' *Journal of Psycholinguistic Research* 10, 27-39.

Atkinson, R.M. 1982. *Explanations in the Study of Child Language Development*. Cambridge: Cambridge University Press.

Bach, E. & Harms, R.T. (eds.). 1968. *Universals in Linguistic Theory*. New York: Holt, Rinehart and Winston.

Bar-Adon, A. & Leopold, W.F. (eds.). 1971. *Child Language: a Book of Readings*. Englewood Cliffs, N.J.: Prentice-Hall.

Barrie-Blackley, S. 1973. 'Six-year-old children's understanding of sentences adjoined with time adverbs'. Journal of Psycholinguistic Research 2, 153-65.

Berko, J. 1958. 'The child's learning of English morphology'. *Word* 14, 150-77.

Bernstein, B. 1971. *Class, Codes and Control I : theoretical studies towards a sociology of language*. London : Routledge & Kegan Paul.
1973. *Class, Codes and Control II : applied studies towards a sociology of language*. London : Routledge & Kegan Paul.

Brener, R. 1983. 'Learning the deictic meaning of third person pronouns.' *Journal of Psycholinguistic Research* 12, 235-62.

Burling, R. 1959. 'Language development of a Garo and English-speaking child.' *Word* 15, 45-68.

Carr, D.B. 1979. 'The development of young children's capacity to judge anomalous sentences. *Journal of Child Language* 6, 227-41.

Chambers, J.C. & Tavuchis, N. 1976. 'Kids and kin: children's understanding of American kin terms.' *Journal of Child Language* 3, 63-80.

Charney, R. 1979. 'The comprehension of here and there.' *Journal of Child Language* 6, 69-80.

Chomsky, C. 1969. *The Acquisition of Syntax in Children from 5 to 10.* Cambridge, Mass. : MIT Press.

Chomsky, N. 1957. *Syntactic Structures.* The Hague: Mouton.
1959. Review of Skinner's *Verbal Behavior.* *Language* 35, 26-58.
1965. *Aspects of the Theory of Syntax.* Cambridge, Mass. : MIT Press.
1972. *Language and Mind (Enlarged Edition).* New York: Harcourt Brace Jovanovich.

Chomsky, N. & Halle, M. 1968. *The Sound Pattern of English.* New York : Harper and Row.

Coker, P.L. 1978. 'Syntactic and semantic factors in the acquisition of before and after'. *Journal of Child Language* 5, 261-77.

Cole, P. & Morgan, J.L. (eds.). 1975. *Syntax and Semantics 3 : Speech Acts.* London and New York : Academic Press.

Conner, P. & Chapman, R.S. 1985. 'The development of locative comprehension in Spanish.' *Journal of Child Language 12*, 109-23.

Corrigan, R. & Odya-Weis, C. 1985. 'The comprehension of semantic relations by two-year-olds: an explanatory study'. *Journal of Child Language 12*, 47-59.

Crystal, D. 1974. Editorial. *Journal of Child Language 1, (i)-(ii).*

Dale, P.S., Loftus, E.F. & Rathbun, L. 1978. 'The influence of the form of the question on the eyewitness testimony of preschool children.' *Journal of Psycholinguistic Research 7*, 269-77.

Deutsch, W. (ed.) 1981. *The Child's Construction of Language.* London : Academic Press.

de Villiers, P A. & de Villiers, J.G. 1972. 'Early judgments of semantic and syntactic acceptability by children'. *Journal of Psycholinguistic Research 1*, 299-310.

Donaldson, M. & McGarrigle, J. 1974. 'Some clues to the nature of semantic development'. *Journal of Child Language 1*, 185-94.

Elliot, A.J. 1981. *Child Language.* Cambridge: Cambridge University Press.

Emerson, H.F. 1979. 'Children's comprehension of "because" in reversible and non-reversible sentences'. *Journal of Child Language 6*, 279-300.

1980. 'Children's judgments of correct and reversed sentences with "if"'. *Journal of Child Language 7*, 137-55.

Emslie, H.C. & Stevenson, R.J. 1981. 'Pre-school children's use of the articles in definite and indefinite referring expressions'. *Journal of Child Language 8*, 313-28.

Feagans, L. 1980. 'How to make sense of temporal/spatial "before" and "after"'. *Journal of Child Language 7*, 529-37.

Fillmore, C.J. 1968. 'The case for case'. In Bach, E. & Harms, R.T. (eds.).

Firth, J.R. 1957. *Papers in Linguistics 1934-1951.* London : O.U.P.

Fremgen, A. & Fay, D. 1980. 'Overextensions in production and comprehension : a methodological clarification.' *Journal of Child Language 7*, 205-11.

Fromberg, D. 1976. 'Syntax model games and language in early education.' *Journal of Psycholinguistic Research 5*, 245-60.

Golinkoff, R.M. & Markessini, J. 1980. '"Mommy sock" : the child's understanding of possession as expressed in two-noun phrases'. *Journal of Child Language 7*, 119-35.

Goodluck, H. 1978. 'Linguistic principles in children's grammar of complement subject interpretation'. *Unpublished Doctoral Dissertation.* University of Massachusetts, Amherst.

Goodluck, H. & Solan, L. (eds.). 1978. 'Papers in the structure and development of child language'. *University of Massachusetts Occasional Papers, Vol. 4.* Linguistics Department, University of Massachusetts, Amherst.

Greenberg, J.H. (ed.). 1963. *Universals in Language.* Cambridge, Mass. : MIT Press.

Grice, H.P. 1975. 'Logic and conversation'. In Cole, P. and Morgan, J.L. (eds.).

Halliday, M.A.K. 1975. *Learning How to Mean.* London: Arnold.

Hamburger, H. & Crain, S. 1982. 'Relative acquisition'. In S.A. Kuczaj II (ed.).

Hargrove, P.M. & Panagos, J.M. 1982. 'Role saliency cues and children's sentence comprehension'. *Journal of Psycholinguistic Research 11*, 217-28.

Harris, P. 1975. 'Inferences and semantic development'. *Journal of Child Language* 2, 143-52.

Haviland, S.E. & Clark, E.V. 1974. '"This man's father is my father's son": a study of the acquisition of English kin terms'. *Journal of Child Language 1*, 23-47.

Hladik, E.G. & Edwards, H.T. 1984. 'A comparative analysis of mother-father speech in the naturalistic home environment'. *Journal of Psycholinguistic Research 13*, 321-32.

Hockett, C.F. 1963. 'The problem of universals in language'. In J.H. Greenberg (ed.).

Horgan, D. 1981. 'Learning to tell jokes : a case study of metalinguistic abilities'. *Journal of Child Language 8*, 217-24.

Howe, C.J. 1981. 'Interpretive analysis and role semantics : a ten-year mésalliance?'. *Journal of Child Language 8*, 439-56.

Howe, Jr., H.E.& Hillman, D. 1973. 'The acquisition of semantic restrictions in children'. *Journal of Verbal Learning and Verbal Behavior 12*, 132-9.

Jakobson, R. & Halle, M. 1956. *Fundamentals of Language.* The Hague : Mouton.

James, S.L. & Miller, J.F. 1973. 'Children's awareness of semantic constraints in sentences'. *Child Development 44*, 69-76.

Johnson, H.L. & Chapman, R.S. 1980. 'Children's judgment and recall of causal connectives : a developmental study of "because", "so" and "and".' *Journal of Psycholinguistic Research 9*, 243-60.

Paul, R. 1985. 'The emergence of pragmatic comprehension : a study of children's understanding of sentence-structure cues to given/new information'. *Journal of Child Language 12,* 161-79.

Piaget, J. 1928. *Judgment and Reasoning in the Child.* London : Kegan Paul.

Rodgon, M., Jankowski, W. & Alenskas, L. 1977. 'A multi-functional analysis of single-word speech.' *Journal of Child Language 4,* 23-43.

Romaine, S. 1984. *The Language of Children and Adolescents: the Acquisition of Communicative Competence.* Oxford : Basil Blackwell.

Schlesinger, I.M. 1981. 'Semantic assimilation in the development of relational categories.' In Deutsch, W. (ed.).

Sheldon, A. 1974. 'The role of parallel function in the acquisition of relative clauses in English'. *Journal of Verbal Learning and Verbal Behavior 13,* 272-81.

Skinner, B.F. 1957. *Verbal Behavior.* New York : Appleton-Century-Crofts.

Slobin, D.I. 1966. 'Grammatical transformations and sentence comprehension in childhood and adulthood'. *Journal of Verbal Learning and Verbal Behavior 5,* 219-27.
1981. 'The origins of grammatical encoding of events.' In Deutsch, W. (ed.).

Smith, N.V. 1973. *The Acquisition of Phonology : A Case Study.* Cambridge : Cambridge University Press.

Solan, L. & Roeper, T. 'Children's use of syntactic structure in interpreting relative clauses'. In Goodluck, H. & Solan, L. (eds.).

Staley, C.M. 1982. 'Sex-related differences in the style of children's language'. *Journal of Psycholinguistic Research 11*, 141-58.

Sully, J. 1896. *Studies of Childhood.* London : Longmans.

Tavakolian, S. 1978. 'The conjoined-clause analysis of relative clauses and other structures'. In Goodluck, H. & Solan, L. (eds.).

Thomson, J.R. & Chapman, R.S. 1977. 'Who is "Daddy" revisited : the status of two-year-olds' over-extended words in use and comprehension'. *Journal of Child Language 4,* 359-75.

Tunmer, W.E., Pratt, C. & Herriman, M.L. 1984. *Metalinguistic Awareness in Children : Theory, Research and Implications.* Berlin : Springer.

Tyler, L.K. & Marslen-Wilson, W. 1978. 'Some developmental aspects of sentence processing and memory'. *Journal of Child Language 5,* 113-29.

Velten, H. 1943. 'The growth of phonemic and lexical patterns in infant language.' *Language 19*, 281-92.

Vygotsky, L.S. 1962. *Thought and Language.* Cambridge, Mass.: MIT Press.

Weir, R. 1962. *Language in the Crib.* The Hague : Mouton.

Wells, G. 1974. 'Learning to code experience through language'. *Journal of Child Language 1,* 243-69.